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Did the 2007 PCAOB Disciplinary Order against Deloitte Impose Actual Costs on the Firm or Improve Its Audit Quality?

Jeff P. Boone
The University of Texas at San Antonio

Inder K. Khurana
University of Missouri

K. K. Raman
The University of Texas at San Antonio

ABSTRACT: We examine whether the December 2007 PCAOB disciplinary order against Deloitte affected Deloitte's switching risk, audit fees, and audit quality relative to the other Big 4 firms over a three-year period following the censure. Our findings suggest that the PCAOB censure was associated with a decrease in Deloitte's ability to retain clients and attract new clients, and a decrease in Deloitte's audit fee growth rates. However, methodologies used in extant archival studies yield little or no evidence to suggest that Deloitte's audit quality was different from that of the other Big 4 firms during a three-year window either before or after the censure. Overall, our results suggest that the PCAOB censure imposed actual costs on Deloitte.

Keywords: *PCAOB censure; switching risk; audit fees; Big 4 audit quality.*

Data Availability: *All data are publicly available.*

I. INTRODUCTION

In this paper, we investigate the nature of the actual costs imposed on Deloitte by the December 2007 disciplinary order issued against the firm by the Public Company Accounting Oversight Board (PCAOB). That is, we examine whether Deloitte's switching risk in terms of the likelihood of losing existing clients to other Big 4 firms or attracting new clients and audit fees

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changed following the PCAOB censure.[1] We also examine whether the PCAOB censure improved Deloitte's audit quality relative to that of the other Big 4 firms during the three years following the PCAOB censure.

The 2002 Sarbanes-Oxley Act (SOX) replaced self-regulation with government oversight of the auditing profession and created the PCAOB, in part, to inspect audit firms and identify deficiencies in audits of publicly owned companies. The Part I public portion of the PCAOB inspection report details the performance deficiencies found in the sample of audit engagements selected by inspectors. Further, each of the Big 4 firms has been publicly identified by the PCAOB inspection reports as having engagement-level audit deficiencies every year since the beginning of inspections in 2004. Lennox and Pittman (2010) indicate that the public portion of the PCAOB inspection reports reveal little or no cross-sectional differences in audit quality among the Big 4 firms, suggesting that these reports have little or no information value for audit market participants.

By contrast, the Part II non-public portion of the inspection report covers defects and criticisms of the auditor's *firm*-level quality controls. In this regard, the December 2007 disciplinary order was unprecedented for a Big 4 firm, representing the first-ever public censure of a Big 4 firm by the PCAOB. These disciplinary proceedings (PCAOB 2007) followed up on a prior-year inspection report by publicly censuring Deloitte for violations of auditing standards in connection with the 2003 audit of Ligand Pharmaceuticals.[2] Notably, the December 2007 censure came about more than three years after Deloitte's resignation from the Ligand engagement in August 2004, and more than two years after the audit failure became public information in May 2005, when Ligand restated its financial statements for 2003–2004. More importantly, unlike the public portion of the PCAOB's inspection reports, which disclose only engagement-level performance weaknesses, the 2007 PCAOB censure publicly revealed for the first time deficiencies in Deloitte's *firm*-level quality controls. Without admitting or denying the PCAOB's findings, Deloitte consented to a $1 million civil penalty and agreed to implement changes to its firm-level quality control policies and procedures (PCAOB 2007, 12). Because of its unique nature, we focus on the December 2007 censure to examine the nature of the actual costs imposed on Deloitte and its impact on the firm's audit quality.[3]

Theoretically, the impact of a PCAOB censure on a Big 4 firm is ambiguous. DeFond and Zhang (2014) note that several SOX-mandated reforms were intended to incentivize audit committees to demand audit quality and shield their companies from perceived threats to audit quality. Relatedly, economic theory (Klein and Leffler 1981; Shapiro 1983) suggests that a PCAOB disciplinary order, by publicly revealing firm-level quality control problems, could damage the Big 4 auditor's brand name reputation for audit quality as perceived by these audit committees, and trigger a decline in the demand for the firm's audit services. The fall in demand would be observable in a change in the firm's switching risk, which is an increased risk of losing an existing client and a decrease in the likelihood of attracting a new client, and/or a decrease in audit fees relative to other Big 4 firms. Potentially, these losses in market share and fees can serve as a deterrent against deficient audits and as an *ex ante* incentive for Big 4 auditors to maintain their brand name service quality (Klein and Leffler 1981; Shapiro 1983).

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[1] We focus on the Big 4 because they represent a separate category of audit firms by virtue of their size and importance in an oligopolistic audit market, such as the fact that they account for over 94 percent of the audit fees paid by public companies (Government Accountability Office [GAO] 2008).

[2] The PCAOB allows an audit firm 12 months or more to remediate quality control issues identified in the *non*-public portion of the inspection report. Hence, the December 2007 censure had to be based on what the PCAOB perceived to be Deloitte's inadequate response to quality control issues identified in a prior year's non-public portion of the inspection report.

[3] Given Deloitte's size, with upward of $20 billion in annual revenues, the $1 million civil penalty was likely immaterial. Hence, our study focuses on the other costs imposed on Deloitte by the 2007 censure. Also, because the December 2007 censure was the first ever, it should be a more salient event as compared to later PCAOB censures of other Big 4 firms that were no longer unique. Hence, our focus on the December 2007 censure potentially provides a stronger quasi-experimental setting for purposes of structuring our tests. Whether the switching risk and audit fee changes following the Deloitte censure were isolated to Deloitte or can be anticipated for other PCAOB censures is beyond the scope of our study.

On the other hand, there are several reasons to doubt that audit committees experienced increased post-SOX sensitivity to audit quality or that the PCAOB has the ability to improve Big 4 audit quality. Specifically, PCAOB Chairman Doty (2011, 1) suggests that because the auditor continues to be hired and fired by the client, audit committees may persist in viewing "their job as negotiating the lowest audit fee" for their client rather than championing audit quality. Also, Palmrose (2006) suggests that PCAOB inspectors may lack state-of-the-art expertise because they are prohibited from being active auditing professionals. In particular, a Big 4 audit implies brand name quality that exceeds the minimum posited by professional standards. Consequently, audit committees may perceive PCAOB inspectors (1) as lacking cutting-edge expertise when it comes to assessing the quality of a brand name Big 4 auditor, and (2) as focused on documentation and substantiation of audit input and process rather than audit outcomes. Such inspectors may focus narrowly on compliance details rather than on forming a holistic assessment of a Big 4 firm's brand name audit quality (Lennox and Pittman 2010). Further, in an oligopolistic audit market, Big 4 clients are potentially faced with a limited choice of auditors (GAO 2003, 2008). Consequently, a PCAOB censure need not necessarily trigger decreased demand for a Big 4 firm's audit services, thereby inflicting a loss by adversely affecting the firm's market share and/or audit fees. Finally, to the extent that PCAOB inspectors lack cutting-edge expertise and/or are narrowly focused on compliance, documentation, and substantiation rather than on audit outcomes, PCAOB oversight may be unrelated to a Big 4 auditor's brand name service quality.[4] For these reasons, the overall effect of a PCAOB censure on the demand for, and quality of, a Big 4 firm's brand name audit services remains an open empirical question.

To examine the consequences of the December 2007 PCAOB disciplinary order, we first examine the changes in Deloitte's switching risk over the 2005–2010 time period relative to that of the other Big 4 firms. Our results using difference-in-differences comparisons indicate that Deloitte's risk of losing (gaining) clients relative to that of other Big 4 auditors increased (decreased) from 2005–2007 to 2008–2010. Further, the increase in the risk of losing existing clients for Deloitte was concentrated during 2008 and 2009, the first two years following the December 2007 PCAOB censure, while the decreased likelihood of gaining new clients spanned all three years. The economic magnitude of these effects was significant as reflected in a 245 (53.1) percent increase (decrease) in loss (gain) risk odds for Deloitte.

Next, we limit our sample to clients with at least two years of tenure with their current auditor to investigate the change in audit fees for Deloitte over the 2005–2010 time period relative to that of other Big 4 firms.[5] Test results indicate that Deloitte moved from imposing audit fee increases that were 3.6 percentage points larger than the other Big 4 during 2005–2007 to imposing audit fee increases during 2008–2010 that were not significantly different from those achieved by the other Big 4. The change in the difference in audit fee growth rates of Deloitte relative to that of the other Big 4 was 2.4 percentage points (3.6 percentage points larger in 2005–2007 versus 1.2 percentage points larger in 2008–2010). This change represents our estimate of the PCAOB censure effect on Deloitte's pricing of continuing audit engagements. Further tests indicate that the post-censure decline in the audit fee growth rate was largely confined to years 2009 and 2010, consistent with

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[4] A PCAOB (2013) staff discussion document suggests that the PCAOB staff have only recently started to look at various audit outcomes as possible indicators of audit quality.

[5] This sample restriction reduces the confounding effects of lowballing on initial audit engagements.

Deloitte curtailing its above-average fee growth rate in these later years in an attempt to stem client defections to other Big 4 auditors following the PCAOB censure.

We also examine Deloitte's audit quality, as proxied by absolute abnormal accruals and the likelihood of financial misstatements as revealed by subsequent restatements, over the 2005–2010 time period relative to that of the other Big 4 firms. We find that Deloitte's audit quality was no different from that of the other Big 4 firms during either the pre-censure (2005–2007) or the post-censure (2008–2010) time periods. Supplementary analysis using the auditor's propensity to issue a going concern opinion as an additional measure of audit quality provided results consistent with the preceding findings. Collectively, these results are consistent with the notion that PCAOB oversight during the 2005–2010 period of our study was focused more on documentation and substantiation compliance than on a holistic assessment of a Big 4 auditor's brand name service quality. Alternatively, it is possible that Deloitte's audit quality improved after the censure, but we were unable to detect the improvement based on existing methods.

Our study contributes to understanding the actual costs imposed by PCAOB oversight on Big 4 firms. To date, the empirical evidence is mixed. For example, Lennox and Pittman (2010) examine the public portion of PCAOB inspection reports for both large and small auditors and find them to be unrelated to clients' auditor switching decisions.[6] By contrast, Abbott, Gunny, and Zhang (2013) examine PCAOB inspection reports for smaller, triennially inspected firms and find that clients of auditors with engagement-level GAAP deficiencies are more likely to switch to other triennially inspected auditors with clean inspection reports. Our study departs from prior research in that we examine the impact of the first-ever PCAOB censure of a Big 4 firm, Deloitte, and find that it imposed actual costs on Deloitte by adversely affecting its switching risk and lowering audit fees. However, Deloitte's audit quality appears to be no different from that of the other Big 4 firms during a three-year period either before or after the censure. Thus, our findings suggest that PCAOB censure can inflict actual harm on a Big 4 auditor by adversely impacting audit committee perceptions of the firm's audit quality, but without an observable improvement in the firm's audit outcomes as reflected in our audit quality metrics.

Our study and Dee, Lulseged, and Zhang (2011) address related, but different research questions and have differing implications. Specifically, while Dee et al. (2011) show that the December 2007 PCAOB action inflicted immediate losses on *investors* in Deloitte clients, our study provides evidence on the longer-term damage suffered by the *auditor* (Deloitte) as measured by a change in the firm's switching risk and a decline in the firm's audit fees over a three-year period following the censure. We also examine the longer-term effects of the PCAOB action on Deloitte's audit quality, which is important considering that the overarching goal of PCAOB oversight is to improve audit quality.

We also contribute to the literature on Big 4 auditor incentives to produce high-quality audits in the U.S. Recent studies (Francis 2011; Lennox and Li 2012; Pritchard 2006) suggest that the limited liability partnership (LLP) form adopted by audit firms and the restrictions on private litigation for securities fraud following the 1995 Litigation Reform Act have effectively lowered the threat of litigation, thereby increasing the importance of reputation protection as an incentive for assuring high-quality audits in the U.S. Other research, including Lennox (1999) and Khurana and Raman (2004), suggests that litigation exposure and not reputation loss drives audit quality in the

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[6] As noted previously, since the beginning of PCAOB inspections in 2004, *all* of the annually inspected larger audit firms, including each of the Big 4, have been identified each year in the public portion of the inspection reports as having engagement-level deficiencies. For example, deficiencies included failing to identify a departure from GAAP in the client financial statements or otherwise failing to obtain sufficient competent evidential matter to support the audit opinion. The PCAOB has identified firms without engagement-level deficiencies only among the triennially inspected smaller audit firms with less than 100 public clients (Gunny and Zhang 2013).

U.K. and the U.S., two of the more litigious countries in the world. As noted by DeFond (2012, 175), empirical research on the incentive effects of auditor reputation loss, particularly in the U.S. context, is scarce, and additional evidence would be fruitful in understanding auditor incentives to provide audit quality. Our findings pertaining to Deloitte's PCAOB censure-related loss of audit market share and fees suggest that reputational loss potentially has a role to play in assuring Big 4 audit quality even in a litigious country such as the U.S.[7] However, our finding of no difference between Deloitte's audit quality and that of the other Big 4 firms around the PCAOB disciplinary order is consistent with the notion that PCAOB inspections during the time period of our study were focused on documentation and substantiation compliance rather than on a holistic assessment of Big 4 audit outcomes or quality. However, such an interpretation is subject to the caveat that our inability to detect any difference in audit quality could be due to the limited power of our tests, including imperfections in our audit quality proxies.

In the next section, we provide background information and develop our hypotheses. Section III discusses the research design and sample. Section IV reports our empirical findings, and Section V provides concluding comments.

II. BACKGROUND AND HYPOTHESES

Background

Section 104 of SOX requires the PCAOB to perform annual inspections of the larger audit firms and triennial inspections of smaller audit firms with fewer than 100 public clients. The statutory regulation, which replaced self-regulation, including peer reviews by professionals from other audit firms, requires inspections by PCAOB inspectors who are prohibited from being active auditing professionals. Consequently, Palmrose (2006) suggests that the switch from self-regulation to the PCAOB essentially represented a trade-off of expertise for perceived independence.

Since the beginning of the PCAOB inspections in 2004, all annually inspected audit firms (including the Big 4) have been cited in the Part I public portion of the PCAOB inspection reports as conducting one or more deficient audits each year based on defects found in a risk-based sample of audit engagements. Lennox and Pittman (2010) indicate that the PCAOB inspection reports have no information value for audit market participants, as measured by subsequent auditor switches and changes in audit firm market shares. They attribute this finding to the fact that although the public portions of these reports, as mandated by SOX, identify engagement-level deficiencies for the inspected audit firms, they do not disclose quality control problems or provide an overall assessment of the firm's audit quality.

In the event that the audit firm fails to address the Board's quality control criticisms in the Part II non-public portion of the inspection report within 12 months, the PCAOB has the option of publicly censuring the audit firm. Further, although the Board has the authority to close an audit firm for failure to make sufficient progress in remedying the quality control deficiencies identified in the non-public portion of its inspection report, the threat of closure is simply not credible against the

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[7] Apart from the PCAOB-imposed $1 million civil penalty, the adverse impact of the PCAOB censure on Deloitte had to be loss of reputation or increased litigation risk. With respect to litigation risk, although our search of the litigation databases through late 2013 disclosed litigation against Ligand, the audit client, following the 2005 restatement, we found *no* information about Ligand-related or PCAOB censure-related litigation against Deloitte. As noted by Francis (2011, 142), the 1995 Litigation Reform Act significantly lowered litigation exposure for auditors in the U.S. and it is not at all unusual for the client, but *not* the auditor, to be sued following a restatement. Further, to the extent that there were any private settlements that were not included in the public litigation databases, these settlements would not be public knowledge and would not be expected to have an effect on the insurance value of a Deloitte audit. Hence, our findings on changes in Deloitte's switching risk and fees suggest that the damage suffered by Deloitte was reputational.

Big 4, which are too large and, after the demise of Arthur Andersen, too few to be put out of business (Norris 2011a, 2011b, 2011c). Consequently, Norris (2011a, 2011b, 2011c) suggests that the Big 4 have often been dismissive of the PCAOB's findings, viewing them as "second guessing" by "government bureaucrats," and have employed stalling tactics to delay or avoid PCAOB censure.

In this paper, we focus on the PCAOB's December 2007 public censure of Deloitte, which was the very first for a Big 4 auditor. In particular, the PCAOB (2007) revealed that Deloitte had failed to comply with professional standards in performing the 2003 audit of Ligand, and to obtain sufficient competent evidential matter to support the unqualified audit opinion that was issued. Further, the PCAOB (2007) alleged that although Deloitte assessed Ligand as a higher-than-normal audit risk engagement, it allowed the engagement partner to continue on the assignment despite serious questions at the top levels of the firm about his competence and proficiency (to the point of determining that he should be removed from all public company audit engagements and counseled to resign from the firm). The PCAOB (2007) also indicated that Deloitte had quality control problems that extended beyond the local office and the Ligand engagement.

Hypotheses Development

Prior research (Klein and Leffler 1981) suggests that damage to reputation can result in economic losses reflecting decreased demand for the firm's services, and this threat provides incentive to maintain brand name product quality. The first type of economic loss that could accrue to Deloitte from the 2007 censure and public disclosure of firm-level quality control problems is a decline in the demand for the firm's audit services. This reduced demand may be exacerbated by audit committees' increased post-SOX sensitivity to audit quality (DeFond and Zhang 2014). Consequently, we expect Deloitte to experience an increase in the rate of clients switching to other Big 4 firms and a decrease in the rate of new clients switching to Deloitte following the PCAOB censure.

However, three considerations could potentially mitigate the change in switching risk for Deloitte following the PCAOB censure. First, audit committees may still be focused on negotiating the lowest fee for a Big 4 audit for their companies (Doty 2011). Second, independence concerns preclude PCAOB inspectors from being active auditing professionals, which limits their expertise and may lower the credibility of the PCAOB inspection findings as perceived by audit committees (DeFond 2010; Palmrose 2006). Further, consistent with the view expressed in the academic literature and practice (Lennox and Pittman 2010), PCAOB inspectors may focus on proper documentation of input and process compliance. As a result, the inspections themselves may be viewed by audit committees as having little to do with Big 4 audit quality. Third, Deloitte consented to the December 2007 censure (without admitting or denying the PCAOB findings) and agreed to establish and implement specific changes in its firm-level quality control procedures and ratings system for audit partners. Further, Deloitte agreed to keep sufficiently detailed records of the implementation of these improvements and have them available for subsequent PCAOB inspections. Potentially, the public disclosure that Deloitte planned to upgrade its quality controls to meet the requirements of future PCAOB inspections could have mitigated the reputational fallout for Deloitte from the PCAOB censure. Our first hypothesis, stated in the null form, is:

> **H1:** There is no change in Deloitte's switching risk relative to other Big 4 firms following the December 2007 PCAOB public censure of Deloitte.

The second type of loss that Deloitte could accrue from the 2007 PCAOB public censure is a decline in audit fees if the firm cut prices to retain audit clients following the damage to its reputation. However, in an oligopolistic audit market, it is also possible for Deloitte's audit fees to increase as the firm established and implemented the quality control upgrades that were agreed upon as part of its settlement with the PCAOB, and managed to pass along these higher costs to its clients. To the extent that audit fees are cost-driven in an oligopolistic market where clients are required by law to have an audit, but have only a limited choice of large auditors (GAO 2003, 2008), an increase in Deloitte's quality control costs could translate into higher fees for its audit clients. Our second hypothesis, also stated in the null form, is:

> **H2:** There is no change in Deloitte's audit fees relative to other Big 4 firms following the December 2007 PCAOB public censure of Deloitte.

Most research to date suggests that the Big 4 brand name implies audit quality in excess of the minimum specified by professional standards. Further, audit quality tends to be higher when audits are performed by firms or offices with industry expertise (Reichelt and Wang 2010) and/or by large offices (Francis and Yu 2009), conditions that exist almost exclusively among the Big 4 firms. The 2007 PCAOB censure implies that Deloitte's audit quality was lower than that of other Big 4 auditors prior to the censure and may be expected to improve after the censure. Alternatively, the 2007 PCAOB censure may be unrelated to Deloitte's Big 4 brand name audit quality because PCAOB inspectors lack cutting-edge technical expertise and because PCAOB inspections are focused on compliance rather than a holistic assessment of audit quality. Thus, the impact of the PCAOB censure on Deloitte's audit quality remains an open empirical question. Our third hypothesis, also stated in the null form, is:

> **H3:** There is no change in Deloitte's audit quality relative to other Big 4 firms between the 2005–2007 pre-censure and the 2008–2010 post-censure periods.

III. RESEARCH DESIGN

Auditor Switch Model

Our first hypothesis focuses on the change in the propensity of Deloitte clients to switch auditors following the PCAOB censure. We analyze the switching activity away from and to Deloitte relative to the other Big 4 auditors by comparing the rate of auditor change to or from Deloitte relative to the rate of auditor change for the other Big 4 publicly traded clients over a six-year period covering three years before and three years after the PCAOB censure. Our choice of the time period reflects the fact that an auditor switch is unlikely to be undertaken lightly because of the substantial costs to the client in the form of search expenses in finding a new auditor, time spent by client executives and other employees in explaining corporate operations and accounting methods to the new auditor, and possibly lower audit quality due to the auditor's steep learning curve in the early years of a new audit.

We perform our tests using the following logistic regression models:

$$\begin{aligned} SWITCH_{i,t} = \beta_{0j} + \sum \beta_k CONTROL_{i,t-1} + \delta_1 DELOITTE\_PY_{i,t} + \delta_2 POST_{i,t} \\ + \delta_3 DELOITTE\_PY_{i,t} \times POST_{i,t} + \varepsilon_{it}. \end{aligned} \tag{1}$$

$$\begin{aligned} SWITCH_{i,t} = \beta_{0j} + \sum \beta_k CONTROL_{i,t-1} + \delta_1 DELOITTE\_CY_{i,t} + \delta_2 POST_{i,t} \\ + \delta_3 DELOITTE\_CY_{i,t} \times POST_{i,t} + \varepsilon_{it}. \end{aligned} \tag{2}$$

SWITCH is equal to 1 if a client changed an auditor between the prior (*t*–1) and current (*t*) year, 0 otherwise; *CONTROL* is a set of control variables used in prior auditor switching studies, described below and defined in Appendix A. *DELOITTE_PY* is equal to 1 if Deloitte was the auditor in the prior (*t*–1) year, and 0 otherwise; *DELOITTE_CY* is equal to 1 if Deloitte was the auditor in the current (*t*) year, and 0 otherwise; and *POST* is equal to 1 for fiscal periods ending between June 1, 2008 and May

31, 2011, and 0 otherwise.[8] Models (1) and (2) include industry dummy variables that allow the intercept to vary for each of the *j* two-digit SIC industry groups (β_{0j}). Both models constrain the post-event change in switching risk to be the same for each of the years 2008–2010. As a specification check, we modify both models to allow the post-event change to differ in 2008, 2009, and 2010. Moreover, we follow Petersen (2009) and Gow, Ormazabal, and Taylor (2010) and calculate the t-statistics based on standard errors that are corrected for auditor-level clustering and heteroscedasticity for these and all subsequent models.[9]

The variable *DELOITTE_PY* (*DELOITTE_CY*) allows the likelihood of Deloitte losing (gaining) a client to differ from that of other Big 4 auditors in the 2005–2007 pre-censure period. The variable *POST* captures the incremental change in switching risk for Big 4 auditors other than Deloitte in the 2008–2010 post-censure period. The variable *DELOITTE_PY* × *POST* (*DELOITTE_CY* × *POST*) captures the incremental change in the likelihood of Deloitte's losing (gaining) a client in the 2008–2010 post-censure period relative to that of other Big 4 auditors. From H1, the coefficient on *DELOITTE_PY* × *POST* is expected to be positive in Model (1) and the coefficient on *DELOITTE_CY* × *POST* is expected to be negative in Model (2) if there is a negative reputational fallout for Deloitte from the PCAOB censure. Alternatively, the coefficient on *DELOITTE_PY* × *POST* (or *DELOITTE_CY* × *POST*) may not be different from zero if the PCAOB censure is perceived by audit committees to have no information value for reasons discussed previously.

Control Variables

The control variables included in our Models (1) and (2) are drawn primarily from Landsman, Nelson, and Rountree (2009) to capture audit and financial risk factors. We include asset *GROWTH* in our model because Stice (1991) finds that growth is indicative of a high audit risk environment. DeFond and Subramanyam (1998) find that large negative discretionary accruals create an incentive for clients to dismiss the incumbent auditor in hopes of finding a more accommodating successor. We use the absolute value of discretionary accruals (*ABSDACC*) to proxy for a client's accounting quality, i.e., higher values of *ABSDACC* imply lower accounting quality, and expect the probability of an auditor switch to increase with *ABSDACC*.[10] We also expect the probability of an auditor switch to increase with *INVREC* because of the higher audit risk associated with higher values of inventories and receivables as a fraction of total assets (Dopuch, Holthausen, and Leftwich 1987; Krishnan 1994).

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[8] We define the pre-censure period as ending in fiscal 2007 in accordance with Compustat convention, so the pre-censure period includes fiscal years that end on or before May 31, 2008, while the post-censure period includes fiscal years that begin on or after June 1, 2008. Although this method of partitioning includes within the pre-censure period a few fiscal periods that end after the December 2007 censure (i.e., fiscal years ending December 31, 2007 through May 31, 2008), we do not view this as a significant problem because auditor retention decisions and audit fee negotiations can be expected to respond to the censure with a significant lag due to recontracting frictions. In contrast, more precise partitioning is required in stock market event tests in order to capture stock price changes in relatively frictionless capital markets. Robustness checks indicate that our results do not change if fiscal years ending December 31, 2007 through May 31, 2008 are included in the post-censure period.

[9] We cluster standard errors at the auditor level because our test variable varies at the auditor level and not the client level. However, if standard errors are clustered by client, then our results and inferences are similar to those reported in the paper (i.e., the coefficient on the test variable was of the same sign with a significance level that yielded the same statistical inference).

[10] *ABSDACC* is the absolute value of the difference between total accruals and the fitted normal accruals estimated by using the following modified Jones (1991) model by fiscal year and two-digit industry SIC code: $\frac{TA_{it}}{Assets_{i,t-1}} = \alpha \frac{1}{Assets_{i,t-1}} + \beta_1 \frac{\Delta SALES_{it} - \Delta AR_{it}}{Assets_{i,t-1}} + \beta_2 \frac{PPE_{it}}{Assets_{i,t-1}} + \beta_3 \frac{IB_{it-1}}{Assets_{i,t-1}} + \varepsilon_{i,t}$, where *TA* is total accruals, calculated as income from continuing operations less operating cash flows from continuing operations; Δ*SALES* is change in sales revenue; Δ*AR* is the change in accounts receivables; *PPE* is gross property and equipment; *IB* is income before discontinued operations and extraordinary items; and the subscripts *i* and *t* denote client and year, respectively. Asset deflated income, or *ROA*, is included to performance-adjust abnormal accruals, as suggested by Kothari, Leone, and Wasley (2005).

Moreover, to the extent that clients with other than a clean opinion (*GC* and *MODOP*) are not satisfied with the current auditor, we expect the probability of an auditor switch to be higher for these clients.

We include auditor *TENURE* as a control variable because shorter tenure results in less client-specific knowledge and a greater likelihood of audit failure (J. Krishnan and J. Krishnan 1997; Stice 1991). However, longer tenure also suggests auditor independence concerns if auditors and clients become too comfortable with each other in long-term relationships. Hence, we do not predict a sign for the *TENURE* variable. To the extent that less profitable companies (*ROA* and *LOSS*) and higher financial *LEVERAGE* firms exhibit more financial risk, we expect the probability of an auditor switch to decrease with *ROA* and to increase with *LOSS* and *LEVERAGE*. Similarly, less *CASH* on hand increases the likelihood of financial difficulty and, hence, the likelihood of an auditor switch (Landsman et al. 2009). We expect the probability of an auditor switch to decrease with higher levels of *CASH*.

We measure the degree of the client-auditor mismatch (*MISMATCH*) using the approach presented in Shu (2000), applied by Landsman et al. (2009), and described in Appendix B. Specifically, we estimate the probability that a client is misaligned with its auditor, in terms of being a Big 4 or a non-Big 4 firm, for each fiscal year.

Because the cost of switching auditors tends to be higher for larger clients (DeAngelo 1981), the probability of an auditor switch is lower for larger clients. Following Landsman et al. (2009), we include variable *M_A* because companies are more likely to switch auditors following a merger/acquisition, especially if the previously independent (but now merged) entities had engaged different auditors. We include annual change (denoted by Δ) in size (*ΔSIZE*) and leverage (*ΔLEVERAGE*) as additional control variables, because prior research (DeFond 1991; Francis and Wilson 1988) notes that changes in agency costs preceding the auditor change could affect the decision to change auditors. Last, we include abnormal audit fee (*ABNRML_FEE*) to control for the effect of pricing pressure on auditor changes, i.e., switches motivated by the desire to pay lower audit fees.

Change in Audit Fees Model

To test H2, we model the change in audit fees as a function of audit risk factors, financial risk factors, and additional control variables. We perform our tests using the following ordinary least squares regression model:

$$\begin{aligned}\Delta AUDIT\_FEE_{i,t} = {} & \beta_{0j} + \sum \beta_k CONTROL_{i,t} + \delta_1 DELOITTE\_CY_{i,t} + \delta_2 POST_{i,t} \\ & + \delta_3 DELOITTE\_CY_{i,t} \times POST_{i,t} + \varepsilon_{i,t}. \end{aligned} \tag{3}$$

ΔAUDIT_FEE is the natural log of change in audit fees in dollars relative to the previous year; *CONTROL* is a set of *k* control variables described in the section below in change form (denoted by Δ), representing the change in a control variable relative to its value in the previous year, and defined in Appendix A. We note that the Appendix A definitions are in levels form. The model includes industry dummy variables that allow the intercept to vary for each of the *j* two-digit SIC industry groups (β_{0j}). We adopt the change-form specification to focus on pricing changes in continuing client engagements. Additionally, the change-form analysis is a "within-company" specification that usually is regarded as offering stronger statistical conclusion validity relative to a levels-form specification.

In Model (3), the variable *DELOITTE_CY* allows the change in audit fees to differ between Deloitte and other Big 4 auditors in the 2005–2007 pre-censure period. The variable *POST* captures the incremental change in audit fees for Big 4 auditors other than Deloitte in the 2008–2010 post-censure period. The variable *DELOITTE_CY* × *POST* captures the incremental change in audit fees for Deloitte in the 2008–2010 post-censure period relative to other Big 4 auditors. From H2, we expect the coefficient on *DELOITTE_CY* × *POST* to be negative if the 2007 PCAOB censure led Deloitte to cut prices in an attempt to retain its audit clients following the damage to its reputation. Alternatively, the coefficient on *DELOITTE_CY* × *POST* could be positive if Deloitte was able to pass along higher-quality costs to audit clients.

Control Variables

We specify control variables in our change in audit fees model in the change form to capture the primary fee determinants as derived from prior audit fee research. The control variables measure the influence of the client size, client complexity, and audit characteristics (Simunic 1980; Hay 2011).

The most dominant determinant of audit fees from prior studies is client *SIZE*, which is expected to have a positive relationship with fees (Simunic 1980). Client complexity is also expected to be positively correlated with audit fees because the more complex the client, the more difficult and time-consuming the audit (Simunic 1980; Hackenbrack and Knechel 1997). We capture client complexity by *SEGMENTS*, *CATA*, and *FOREIGN*. We also control for client's financial risk by *QUICK*, *LEVERAGE*, *ROA*, and *LOSS* (Johnstone and Bedard 2004; Choi, Doogar, and Ganguly 2004; Hogan and Martin 2009). Greater client risk is reflected in lower profitability and the quick ratio, as well as in higher leverage and by the presence of a loss, and produces higher expected audit fees. Because audit fees are expected to be higher for companies that receive a going concern or an otherwise modified opinion, we include *GC* and *MODOP* as additional control variables. Finally, we include prior-year abnormal audit fee (*ABNRML_FEE*) to control for the effect of pricing pressure on audit fee growth rates. Unlike the other control variables in Model (3), *ABNRML_FEE* enters the model in level form.

Audit Quality

To test H3, we infer audit quality by examining (1) client's earnings management behavior with respect to abnormal accruals (Francis, Maydew, and Sparks 1999), and (2) restatements due to failures in the application of GAAP (Palmrose and Scholz 2004; Francis and Michas 2013).[11] Specifically, we estimate the following model using ordinary least squares regression for abnormal accruals and a logistic regression for the probability of client restatement:

$$\begin{aligned} ABSDACC_{i,t}(\text{or } RESTATE_{i,t}) = \beta_{0j} + \sum \beta_k CONTROL_{i,t} + \delta_1 DELOITTE\_CY_{i,t} + \delta_2 POST_{i,t} \\ + \delta_3 DELOITTE\_CY_{i,t} \times POST_{i,t} + \varepsilon_{it}, \end{aligned} \tag{4}$$

where *ABSDACC* represents the unsigned asset-deflated, performance-adjusted abnormal accruals defined previously; and *RESTATE* is equal to 1 for observations where the client subsequently restates the current-year financial statements due to an accounting rule application error (excluding clerical errors), and 0 otherwise.[12] *CONTROL* is a set of *k* control variables described in the section below, and defined in Appendix A, and all other variables are defined as before. The model includes industry dummy variables that allow the intercept to vary for each of the *j* two-digit SIC industry groups (β_{0j}).

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[11] Because audit quality cannot be directly observed (Francis 2011), we examine both abnormal accruals and restatements to capture continuous, as well as discrete measures of audit quality. As noted by DeFond and Zhang (2014), the ultimate goal of auditing is to improve financial reporting quality as reflected in earnings quality metrics such as abnormal accruals. In other words, they suggest that the continuous nature of abnormal accruals is consistent with the continuous nature of audit quality. By contrast, although restatements are indicative of "black and white" audit failures, they fail to capture subtle variations in audit quality (DeFond and Zhang 2014).

[12] We classify a restatement as a non-clerical-error application error based on the Audit Analytics (WRDS version) variables *RES_ACCOUNTING* (must equal 1) and *RES_CLER_ERR* (must equal 0). Based on the Audit Analytics manual, Audit Analytics, in creating these two variables, follows a protocol that closely resembles the algorithm suggested by Hennes, Leone, and Miller (2008): classify a restatement as a non-clerical-error application error if the firm explicitly uses variants of "fraud" or "irregularity" in discussion of the restatement, if there is a related Securities and Exchange Commission (SEC) Division of Enforcement or DOJ investigation, or if there is a non-SEC independent investigation into the misstatement.

In Model (4), the variable *DELOITTE_CY* × *POST* captures the incremental change in audit quality for Deloitte in the 2008–2010 post-censure period relative to other Big 4 auditors. Under H3, we expect the coefficient on *DELOITTE_CY* × *POST* to be negative if the 2007 PCAOB censure was followed by an improvement in Deloitte's audit quality. Alternatively, the coefficient on *DELOITTE_CY* × *POST* could be positive if the 2007 censure was followed by an unintended deterioration in Deloitte's audit quality.

Control Variables

We identify a parsimonious set of variables used in prior studies to control for various auditor and client characteristics that affect a company's level of abnormal accruals or the likelihood of client restatements. The auditor-related variables we consider are *INFLUENCE, SHORT, AUDIT_FEE, NONAUDIT_FEE, EFFORT, SEC_REG_OFF,* and *LNPOP*. Francis and Yu (2009) use the variable *INFLUENCE* to capture the importance of a client to the local practice office, and argue that this variable can affect auditor objectivity and audit quality for that client. However, they do not find this variable to be significant in most of their analyses, so we do not predict a sign for the coefficient on *INFLUENCE*. Johnson, Khurana, and Reynolds (2002) argue and find that a short tenure of the auditor is related to a higher magnitude of discretionary accruals (lower audit quality). Therefore, the predicted sign for *SHORT* is positive. Kinney, Palmrose, and Scholz (2004) argue that high levels of fees have the potential to create an economic bond that gives the client leverage over the auditor and may reduce the auditor's objective and professional skepticism. Hence, we predict a positive sign for both *AUDIT_FEE* and *NONAUDIT_FEE* variables. Prior research also suggests that greater audit effort can curb earnings management (Caramanis and Lennox 2008); therefore, we expect a negative sign on *EFFORT*. We also control for whether the auditor is located in the same city as an SEC regional office because Kedia and Rajgopal (2011) document that misreporting is more likely to occur for companies located farther away from SEC regional offices. Hence, we expect a positive sign on *SEC_REG_OFF*. We also include *LNPOP* as a proxy for city size where the auditor is located, because auditors in larger cities are likely to exhibit more accounting expertise to catch accounting errors. Hence, we expect a negative sign on *LNPOP*.

Client-level variables we consider are *SIZE, SEGMENTS, LOSS, GROWTH, MB, LEVERAGE, BANKRUPTCY, SALES_VOLATILITY, CFO, CFO_VOLATILITY, LAGGED_ACCRUALS, ISSUE,* and *LITRISK*. Based on prior research, we expect *SIZE, BANKRUPTCY, LAGGED_ACCRUALS, CFO,* and *LOSS* to be negatively associated with abnormal accruals, while we expect *CFO_VOLATILITY, SALES_VOLATILITY, GROWTH, LEVERAGE, ISSUE, MB,* and *LITRISK* to be positively associated with abnormal accruals (Choi, C. Kim, J. Kim, and Zang 2010; Francis and Yu 2009; Hribar and Nichols 2007). We do not predict a sign for *SEGMENTS* due to absent or conflicting results in prior studies (Francis and Yu 2009).

IV. SAMPLE AND RESULTS

Sample

We use four samples to test the study's hypotheses, as summarized in Table 1. Our initial sample consists of 57,805 client-year observations in Audit Analytics with fiscal years between 2005 and 2010. We then exclude 26,039 client-years not in Compustat, 6,201 banking and financial services client-years, 7,998 client-years not using a Big 4 auditor in the current year, 6,853 client-

TABLE 1
Sample Formation

	Auditor Switch Sample		Audit Fee Sample	Abnormal Accrual and Restatement Sample
	Gains	Losses		
Client-year observations with complete panel data 2005–2010 and with auditor office located in MSA	57,805	57,805	57,805	57,805
Exclude observations not in Compustat	−26,039	−26,039	−26,039	−26,039
Exclude banking and financial service firms	−6,201	−6,201	−6,201	−6,201
Exclude observations not using a Big 4 auditor in current year	−7,998	−7,998	−7,998	−7,998
Exclude observations missing Compustat data	−6,853	−6,853	−7,553	−11,114
Exclude observations with fewer than 15 industry-year observations available to estimate abnormal accruals	−701	−701		−701
Exclude observations not using Big 4 auditor in prior year		−43	−43	−43
Sample used	10,013	9,970		
Exclude observations not using the same Big 4 auditor in current and prior year			−205	
Sample used			9,766	5,709

years with missing data, and 701 client-years with insufficient industry-level data to estimate the abnormal accruals model. These screens result in a client sample of 10,013 firm-year observations. Included within this sample are 9,970 observations that used a Big 4 auditor in both the current and prior year, and 43 observations that used a non-Big 4 auditor last year and a Big 4 auditor in the current year. We use the entire sample of 10,013 observations to analyze the ability of Deloitte to attract new clients from Big 4 or other audit firms. We use the smaller sample of 9,970 observations to analyze the ability of Deloitte to avoid losing existing clients to other Big 4 auditors.[13] The remaining two samples are subsets of this auditor switch sample. For the audit fee sample, to make audit fee independent of the switching decision, we require the client to use the same Big 4 auditor in successive years, which eliminates 248 observations and results in a sample of 9,766 firm-year observations. Additional data constraints reduce the sample size to 5,709 for the abnormal accrual and restatement sample.

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[13] Audit Analytics classifies auditor switch observations as auditor resignations or client dismissals based on information within 8-K filings. Of the switch observations in our sample, 9.8 percent represent auditor resignations, while the remaining 90.2 percent represent client dismissals. As in Landsman et al. (2009), we recognize that this two-way classification does not necessarily capture the underlying dynamics of the auditor switch. Clients may dismiss an auditor in anticipation of a resignation to avoid the negative reaction by the market or regulators. Moreover, auditors may force a client dismissal by increasing fees to an unrealistic level for that particular client. They also may prefer to be dismissed because a resignation could harm their ability to attract new clients. For this reason, we do not use this classification in our tests. As a sensitivity test, we repeat the analysis after excluding observations in which the auditor resigned, and find that our results are similar to those reported in the paper (i.e., the coefficient on the test variable was of the same sign with a significance level that yielded the same statistical inference).

In an attempt to mitigate concerns that omitted correlated variables are driving our results, we also rely on propensity score matched (PSM) samples to test our hypotheses.[14] For the PSM approach, we match on specific client characteristics, such as client size, leverage, return on assets, sales growth, cash to total assets, asset turnover, and two-digit SIC industry code, to identify whether Deloitte versus other Big 4 differences can be attributed to specific client characteristics. In the first step, a logit model is estimated to predict the probability that a firm is audited by Deloitte, conditional on the matching characteristics. We then match, without replacement, clients that are audited by the other Big 4, based on closeness to the predicted value from the first step. We use a narrow caliper distance of 0.03 to closely match the two samples on client characteristics. The resulting matched samples range from 4,132 to 4,418 observations.

Finally, we note that Ernst & Young (E&Y) and PricewaterhouseCoopers (PwC) were also subject to PCAOB censure later during the test period. Under our alternative hypothesis, these censures would narrow the difference between Deloitte and the other Big 4 and bias against rejecting the null form of our hypotheses. However, to mitigate concerns that the censures against other Big 4 firms are influencing our results, we repeat our tests after excluding E&Y and PwC. In other words, we use KPMG as the control group and the tests become a comparison of Deloitte to KPMG. The resulting sample sizes ranged from 4,052 to 4,218 observations.[15]

Empirical Results

Descriptive Statistics

Table 2 presents a descriptive analysis of changes in Deloitte's share of the audit market and Deloitte client attrition rates during the period 2005–2010 for the full sample of 57,805 client-year observations. Our sample includes 10,295 companies in 2005, of which 1,509 companies were Deloitte clients, for a 2005 Deloitte market share of 14.7 percent. Between 2005 and 2006, 1,342 companies exited the sample, of which 136 were Deloitte clients. This left 8,953 (10,295 – 1,342) and 1,373 (1,509 – 136) continuing clients from 2005 for the population and Deloitte, respectively. Of these 1,373 continuing Deloitte clients, 76 switched to other auditors during 2006, yielding a 5.5 percent Deloitte attrition rate (0.055 = 76/1,373) for 2006. In addition, during 2006, Deloitte added 106 out of the 1,027 companies that are new to the sample and 44 companies that previously were served by other auditors. These changes together resulted in a 2006 sample of 9,980 total companies, of which 1,447 were Deloitte clients, yielding a 2006 Deloitte market share of 14.5 percent (0.145 = 1,447/9,980).

When we extend this analysis to the entire time period of the study, we find in Table 2 that Deloitte's overall market share declined from 14.7 percent in 2005 to 13.7 percent in 2010. The Deloitte losses in continuing clients imply attrition rates of 5.5 percent, 4.3 percent, 3.1 percent, 6.4 percent, and 2.7 percent for the years 2006–2010, respectively. Attrition rates in year 2009 evidence a clear spike, breaking an otherwise downward across-time trend. In untabulated tests, the year 2009 attrition rate is significantly higher than the 2007, 2008, and 2010 attrition rates ($p < 0.05$), but insignificantly different from the 2006 attrition rate ($p > 0.10$). In comparison, the untabulated attrition rate of the other Big 4 firms for 2009 is 3.04 percent. Since we have defined the years in accordance with Compustat convention, fiscal year 2009 includes accounting years

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[14] PSM is an econometric method that allows for efficient matching of treatment firms with a set of control firms (Rosenbaum and Rubin 1983).

[15] Results using these non-censure KPMG samples as control group yielded inferences similar to those reported in the paper (i.e., the coefficient on the test variable was of the same sign with a significance level that yielded the same statistical inference).

TABLE 2

Attrition in Deloitte Clients

Panel A: Attrition Years 2005–2007

		2005					2006					2007				
		Total	Deloitte				Total	Deloitte				Total	Deloitte			
Start	End	n	+	−	n	%	n	+	−	n	%	n	+	−	n	%
2005	2010	5,829			989	17.0%	5,829	32	49	972	16.7%	5,829	24	32	964	16.5%
2005	2009	708			89	12.6%	708	2	5	86	12.1%	708	4	6	84	11.9%
2005	2008	479			63	13.2%	479	3	4	62	12.9%	479	2	4	60	12.5%
2005	2007	746			99	13.3%	746	4	4	99	13.3%	746	0	12	87	11.7%
2005	2006	1,191			133	11.2%	1,191	3	14	122	10.2%	7,762			1,195	15.4%
2005	2005	1,342			136	10.1%	8,953			1,341	15.0%					
		10,295			1,509	14.7%										
2006	2010						509			73	14.3%	509	0	2	71	13.9%
2006	2009						129			17	13.2%	129	0	0	17	13.2%
2006	2008						88			6	6.8%	88	0	1	5	5.7%
2006	2007						132			5	3.8%	132	0	0	5	3.8%
2006	2006						169			5	3.0%	858			98	11.4%
							1,027			106	10.3%					
2007	2010											605			65	10.7%
2007	2009											151			10	6.6%
2007	2008											153			5	3.3%
2007	2007											160			11	6.9%
												1,069			91	8.5%
Total		10,295	0	0	1,509	14.7%	9,980	44	76	1,447	14.5%	9,689	30	57	1,384	14.3%
Continuing clients from prior year										1,373					1,320	
Attrition rate (losses/continuing from prior year)										0.055					0.043	

(continued on next page)

TABLE 2 (continued)

Panel B: Attrition Years 2008–2010

		2008					2009					2010				
		Total	Deloitte				Total	Deloitte				Total	Deloitte			
Start	End	n	+	−	n	%	n	+	−	n	%	n	+	−	n	%
2005	2010	5,829	21	32	953	16.3%	5,829	28	60	921	15.8%	5,829	12	26	907	15.6%
2005	2009	708	2	3	83	11.7%	708	4	11	76	10.7%					
2005	2008	479	1	4	57	11.9%	6,537			997	15.3%					
2005	2007	7,016			1,093	15.6%										
2006	2010	509	1	0	72	14.1%	509	1	1	72	14.1%	509	2	0	74	14.5%
2006	2009	129	0	0	17	13.2%	129	1	1	17	13.2%					
2006	2008	88	0	0	5	5.7%	638			89	13.9%					
2006	2007	726			94	12.9%										
2007	2010	605	0	1	64	10.6%	605	1	3	62	10.2%	605	2	1	63	10.4%
2007	2009	151	0	0	10	6.6%	151	0	0	10	6.6%					
2007	2008	153	0	0	5	3.3%	756			72	9.5%					
2007	2007	909			79	8.7%										
2008	2010	607			45	7.4%	607	1	4	42	6.9%	607	1	2	41	6.8%
2008	2009	178			7	3.9%	178	1	0	8	4.5%					
2008	2008	182			3	1.6%	785			50	6.4%					
		967			55	5.7%										
2009	2010						634			66	10.4%	634	0	2	64	10.1%
2009	2009						185			4	2.2%					
							819			70	8.5%					
2010	2010											504			41	8.1%
Total		9,618	25	40	1,321	13.7%	9,535	37	80	1,278	13.4%	8,688	17	31	1,190	13.7%

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TABLE 2 (continued)

		2008					2009					2010				
		Total	Deloitte				Total	Deloitte				Total	Deloitte			
Start	End	n	+	−	n	%	n	+	−	n	%	n	+	−	n	%
Continuing clients from prior year					1,281					1,251					1,163	
Attrition rate (losses/continuing from prior year)					0.031					0.064					0.027	

that end between June 2009 and May 2010. Assuming that auditor changes occur early in the fiscal year, we conclude that the attrition rate spike in year 2009 for Deloitte potentially relates to auditor changes that occurred during July 2008 to June 2009 after the December 2007 censure by the PCAOB.

Table 3 reports descriptive statistics for the variables used in the empirical analysis. Panels A–C involve the sample used in the switching risk, audit fee, and audit quality analyses, respectively. In Table 3, Panel A, the mean values of the client-year variables indicate an auditor switching rate of 2.17 percent. There is a 14 percent growth in assets (*GROWTH*), absolute abnormal accruals (*ABSDACC*) of 6 percent of total assets, and 22 percent of total assets invested in receivables and inventory (*INVREC*). Sample clients experienced a 2 percent rate of going concern opinions (*GC*), 55 percent rate of modified audit opinions excluding going concern (*MODOP*), which is consistent with Landsman et al. (2009).[16] Mean auditor tenure (*TENURE*) is 11.52 years. The mean and median value of return on assets (*ROA*) leverage for our sample clients were 0 and 4 percent, respectively, suggesting that large losses during our sample years reduced the mean value of *ROA*. Approximately 28 percent of the client-year observations exhibited a net loss (*LOSS*). Client-years were moderately leveraged, with mean *LEVERAGE* of 21 percent, and significant cash holdings with a mean value of *CASH* of 22 percent. Only 20 percent of the observations reflected poor auditor-client alignment (mean value of *MISMATCH*). Deloitte served as auditor in 22 percent of the firm-years (mean value of *DELOITTE_CY*), and 47 percent of the observations relate to the post-event (censure) years 2008–2010 (mean value of *POST*).

Turning next to Table 3, Panel B, Δ*AUDIT_FEE* shows a 5 percent growth in audit fees on continuing clients (mean value Δ*AUDIT_FEE*). The central tendencies of all of the control variables measured in n year-to-year change-form are near zero with two exceptions. Δ*SIZE* shows a 7 percent growth in assets (mean value of Δ*SIZE*), and Δ*QUICK* shows a 7 percent decline in the quick ratio (mean value of Δ*QUICK*). In Table 3, Panel C, *ABSDACC* has a mean (median) value of 0.06 (0.039), which is similar to other studies (e.g., Reichelt and Wang 2010; Reynolds and Francis 2000). About 5 percent of the sample client-year observations are restated. The mean value of 5.73 percent for *INFLUENCE* indicates that the average client represents 5.7 percent of total fees charged to all clients of an office. About half of the auditor offices of the clients in our sample are not in the same city as an SEC regional office. Several client characteristics exhibit mean values similar to those reported in Panel A of Table 3 for the larger sample.

Inspection of the correlations presented in Table 4, Panels A–E show no large correlations that could induce collinearity problems in our multivariate analysis, and we confirm this inference by examining the Variance Inflation Factors (VIFs), which never exceeded 5 for any of our explanatory variables in any of the regressions.

Switching Risk Analysis

Logit regression results for two alternative specifications of Models (1) and (2) are presented in Table 5, Panel A. Columns (1) and (2) relate to the auditor switch (loss) sample and columns (3) and (4) relate to the auditor switch (gain) sample. The difference between the specifications in

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[16] *MODOP* is measured based on the Compustat item mnemonic AUOP, which is flagged for *any* deviation from a straight, unqualified opinion. For example, an unqualified opinion with additional explanatory language would be considered a modified opinion. This definition is consistent with Landsman et al. (2009), and our sample mean of *MODOP* (55 percent) is identical to that reported in Landsman et al.'s (2009) Table 2 for the non-switch post-Enron period.

TABLE 3
Descriptive Statistics

Panel A: Auditor Switch Analysis

	n	Mean	Median	Std. Dev.
SWITCH	10,013	0.0217	0.0000	0.1456
GROWTH	10,013	0.1437	0.0646	0.3922
ABSDACC	10,013	0.0607	0.0429	0.0583
INVREC	10,013	0.2242	0.1923	0.1665
GC	10,013	0.0163	0.0000	0.1266
MODOP	10,013	0.5497	1.0000	0.4976
TENURE	10,013	11.5153	8.0000	9.1822
ROA	10,013	−0.0003	0.0401	0.1744
LOSS	10,013	0.2816	0.0000	0.4498
LEVERAGE	10,013	0.2094	0.1649	0.2162
ΔLEVERAGE	10,013	0.0036	0.0000	0.0850
CASH	10,013	0.2195	0.1275	0.2328
MISMATCH	10,013	0.2027	0.0000	0.4021
SIZE	10,013	6.7394	6.6522	1.8263
ΔSIZE	10,013	0.0639	0.0511	0.2369
M_A	10,013	0.2905	0.0000	0.4540
ABNRML_FEE	10,013	0.0209	0.0000	0.4728
DELOITTE_CY	10,013	0.2171	0.0000	0.4123
DELOITTE_PY	9,970	0.2169	0.0000	0.4121
POST	10,013	0.4682	0.0000	0.4990

Panel B: Change in Audit Fee Analysis

	n	Mean	Median	Std. Dev.
ΔAUDIT_FEE	9,766	0.0528	0.0161	0.3251
ΔSIZE	9,766	0.0669	0.0502	0.2621
Δln(SEGMENTS)	9,766	−0.0013	0.0000	0.1953
ΔCATA	9,766	−0.0029	0.0024	0.0882
ΔFOREIGN	9,766	0.0076	0.0000	0.0784
ΔQUICK	9,766	−0.0740	0.0000	1.3540
ΔLEVERAGE	9,766	0.0006	0.0000	0.1046
ΔROA	9,766	−0.0015	0.0009	0.1030
ΔLOSS	9,766	0.0034	0.0000	0.4242
ΔGC	9,766	0.0067	0.0000	0.1419
ΔMODOP	9,766	0.0004	0.0000	0.5751
ABNRML_FEE	9,766	−0.0004	−0.0002	0.5013
DELOITTE_CY	9,766	0.2264	0.0000	0.4185
POST	9,766	0.4689	0.0000	0.4991

Panel C: Abnormal Accruals and Restatement Analysis

	n	Mean	Median	Std. Dev.
ABSDACC	5,709	0.0602	0.0397	0.0673
RESTATE	5,709	0.0487	0.0000	0.2152
INFLUENCE	5,709	0.0573	0.0198	0.1127

(continued on next page)

TABLE 3 (continued)

	n	Mean	Median	Std. Dev.
SHORT	5,709	0.0687	0.0000	0.2529
AUDIT_FEE	5,709	14.2002	14.0999	1.0786
NONAUDIT_FEE	5,709	10.7336	12.0611	5.2340
EFFORT	5,709	58.9716	51.8437	33.2380
SEC_REG_OFF	5,709	0.5062	1.0000	0.5000
LNPOP	5,709	15.2127	15.3172	0.8226
SIZE	5,709	6.7266	6.6345	1.8548
SEGMENTS	5,709	2.0513	1.0000	1.6447
LOSS	5,709	0.2811	0.0000	0.4496
GROWTH	5,709	0.0723	0.0480	0.2307
MB	5,709	0.9035	0.8342	0.7722
LEVERAGE	5,709	0.1759	0.1428	0.1775
BANKRUPTCY	5,709	4.4251	3.4009	5.7272
SALES_VOLATILITY	5,709	0.1691	0.1055	0.1920
CFO	5,709	0.0828	0.0994	0.1571
CFO_VOLATILITY	5,709	0.0612	0.0396	0.0733
LAGGED_ACCRUALS	5,709	−0.0727	−0.0586	0.1007
ISSUE	5,709	0.6441	1.0000	0.4788
LITRISK	5,709	0.2757	0.0000	0.4469
DELOITTE_CY	5,709	0.1960	0.0000	0.3970
POST	5,709	0.4882	0.0000	0.4999

Δ represents a change in a variable relative to its value in the previous year.
See Appendix A for other variable definitions.

columns (1) and (3) versus (2) and (4) reflect how the post-event change in switching risk is modeled. The specification in columns (1) and (3) constrains the post-event change to be the same for each of the years 2008–2010, while the specification in columns (2) and (4) allows that change to differ in 2008, 2009, and 2010.

The control variables in Table 5, Panel A are generally significant, with signs consistent with prior research and in accordance with economic intuition. We note that our control for fee pressure, *ABNRML_FEE*, loads with positive and strongly significant coefficients, which suggests that companies paying above (below) normal audit fees are at a higher (lower) risk of changing auditors the following year. This control is particularly important in light of our evidence (discussed later) that Deloitte enjoyed pricing premia in the 2005–2007 pre-censure period that might manifest in 2008–2010 client loss as companies sought services from lower-priced auditors.

In terms of our hypothesis, with respect to both auditor switch (loss and gain) samples, the coefficients δ_1–δ_3 in Table 5, Panel A, columns (1) and (3), and the coefficients δ_1 and δ_4–δ_9 in columns (2) and (4) are of interest. The coefficient on *DELOITTE_PY* × *POST* in column (1) is positive and statistically significant ($p < 0.01$), indicating that the likelihood of a client switch away from Deloitte (relative to the other Big 4) widened in the post-censure period. This heightened switching risk of Deloitte during the post-event period was primarily concentrated in the first two years following the PCAOB sanction, as indicated by the coefficients on *DELOITTE_PY* × *YR08*

TABLE 4

Pearson Correlations

Panel A: Auditor Switch Analysis Variables *SWITCH* to *LEVERAGE*

		1	2	3	4	5	6	7	8	9	10
1	*SWITCH*	1.00									
2	*GROWTH*	0.01	1.00								
3	*ABSDACC*	0.03	0.20	1.00							
4	*INVREC*	0.01	−0.08	−0.04	1.00						
5	*GC*	0.01	−0.08	0.08	−0.03	1.00					
6	*MODOP*	0.01	−0.03	−0.04	−0.01	0.11	1.00				
7	*TENURE*	−0.03	−0.13	−0.13	0.14	0.00	0.08	1.00			
8	*ROA*	−0.01	0.14	−0.25	0.19	−0.24	0.01	0.13	1.00		
9	*LOSS*	0.03	−0.12	0.20	−0.14	0.18	0.00	−0.13	−0.71	1.00	
10	*LEVERAGE*	0.01	−0.05	−0.08	−0.14	0.10	0.08	−0.02	−0.09	0.09	1.00
11	*ΔLEVERAGE*	0.01	0.03	0.01	−0.03	−0.02	0.01	−0.03	−0.02	−0.01	−0.16
12	*CASH*	−0.01	0.13	0.30	−0.30	0.01	−0.10	−0.15	−0.36	0.30	−0.35
13	*MISMATCH*	0.00	−0.03	0.18	0.02	0.08	−0.07	−0.14	−0.32	0.28	−0.23
14	*SIZE*	−0.02	0.09	−0.16	−0.15	−0.18	0.14	0.24	0.38	−0.38	0.07
15	*ΔSIZE*	−0.02	−0.10	0.00	0.02	0.01	−0.14	0.01	−0.01	0.02	0.03
16	*M_A*	0.01	0.17	0.02	0.00	−0.05	0.02	−0.04	0.06	−0.07	0.00
17	*ABNRML_FEE*	0.04	0.01	−0.02	0.01	0.00	0.01	−0.01	0.01	−0.02	0.06
18	*DELOITTE_CY*	0.01	0.00	−0.04	0.03	−0.02	−0.01	−0.05	0.06	−0.07	0.02
19	*DELOITTE_PY*	0.01	0.00	−0.05	0.03	−0.02	−0.01	−0.05	0.06	−0.06	0.02
20	*POST*	−0.01	−0.11	0.03	−0.04	0.00	0.20	0.07	−0.05	0.08	0.06

Panel B: Auditor Switch Analysis Variables *ΔLEVERAGE* to *POST*

		11	12	13	14	15	16	17	18	19	20
1	*SWITCH*										
2	*GROWTH*										
3	*ABSDACC*										
4	*INVREC*										
5	*GC*										
6	*MODOP*										
7	*TENURE*										
8	*ROA*										
9	*LOSS*										
10	*LEVERAGE*										
11	*ΔLEVERAGE*	1.00									
12	*CASH*	0.02	1.00								
13	*MISMATCH*	0.00	0.40	1.00							
14	*SIZE*	0.03	−0.25	−0.55	1.00						
15	*ΔSIZE*	−0.24	−0.05	0.01	−0.08	1.00					
16	*M_A*	0.00	−0.13	−0.11	0.08	−0.02	1.00				
17	*ABNRML_FEE*	−0.02	−0.07	0.01	0.02	0.00	0.02	1.00			
18	*DELOITTE_CY*	0.01	−0.09	−0.03	0.02	0.00	−0.03	0.01	1.00		
19	*DELOITTE_PY*	0.01	−0.10	−0.04	0.03	0.00	−0.03	0.01	0.97	1.00	
20	*POST*	−0.05	−0.03	0.07	0.01	−0.08	0.01	0.00	0.00	0.01	1.00

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TABLE 4 (continued)

Panel C: Change in Audit Fee Analysis

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1 *ΔAUDIT_FEE*	1.00													
2 *ΔSIZE*	0.24	1.00												
3 *Δln(SEGMENTS)*	0.05	0.05	1.00											
4 *ΔCATA*	−0.11	−0.18	−0.05	1.00										
5 *ΔFOREIGN*	0.02	0.02	−0.03	0.01	1.00									
6 *ΔQUICK*	−0.05	0.13	−0.04	0.39	0.00	1.00								
7 *ΔLEVERAGE*	0.06	0.05	0.03	−0.14	−0.01	0.03	1.00							
8 *ΔROA*	−0.03	0.24	−0.01	0.13	−0.03	0.21	−0.12	1.00						
9 *ΔLOSS*	0.06	−0.09	0.02	−0.07	0.04	−0.08	0.09	−0.35	1.00					
10 *ΔGC*	0.02	−0.12	−0.01	−0.02	0.04	−0.10	−0.12	−0.15	0.07	1.00				
11 *ΔMODOP*	0.08	0.07	−0.01	−0.04	0.02	−0.02	0.02	−0.04	0.02	0.09	1.00			
12 *ABNRML_FEE*	−0.37	0.02	0.01	0.04	0.03	−0.01	−0.04	−0.01	0.03	0.01	0.03	1.00		
13 *DELOITTE_CY*	0.03	0.00	0.00	0.01	−0.01	0.01	0.01	−0.01	0.01	0.00	−0.02	0.00	1.00	
14 *POST*	−0.22	−0.15	−0.01	0.03	0.00	0.04	−0.03	0.01	0.01	0.00	−0.31	0.00	0.01	1.00

Panel D: Abnormal Accruals and Restatement Analysis (*ABSDACC* to *LOSS*)

	1	2	3	4	5	6	7	8	9	10	11	12
1 *ABSDACC*	1.00											
2 *RESTATE*	−0.01	1.00										
3 *INFLUENCE*	−0.08	0.00	1.00									
4 *SHORT*	0.03	−0.01	−0.02	1.00								
5 *AUDIT_FEE*	−0.18	0.02	0.39	−0.02	1.00							
6 *NONAUDIT_FEE*	−0.11	0.01	0.17	−0.13	0.37	1.00						
7 *EFFORT*	0.04	0.03	0.13	0.04	0.49	0.08	1.00					
8 *SEC_REG_OFF*	0.02	−0.01	0.22	0.00	−0.06	−0.06	−0.07	1.00				
9 *LNPOP*	−0.02	0.00	−0.30	0.00	0.17	0.09	0.09	−0.70	1.00			
10 *SIZE*	−0.23	0.00	0.38	−0.03	0.85	0.38	−0.01	−0.02	0.13	1.00		
11 *SEGMENTS*	−0.08	−0.01	0.14	−0.02	0.33	0.16	0.14	−0.02	0.07	0.29	1.00	
12 *LOSS*	0.25	0.02	−0.09	0.03	−0.23	−0.16	0.10	0.04	−0.07	−0.32	−0.14	1.00
13 *GROWTH*	0.05	−0.02	−0.01	−0.01	−0.02	0.02	−0.14	0.01	0.00	0.07	−0.04	−0.28
14 *MB*	0.05	−0.03	−0.01	−0.03	0.01	0.02	0.05	0.01	−0.01	−0.02	−0.09	−0.07
15 *LEVERAGE*	−0.05	0.01	0.16	0.04	0.26	0.12	−0.09	−0.04	0.05	0.37	0.07	0.03
16 *BANKRUPTCY*	−0.10	−0.04	−0.07	−0.02	−0.14	−0.06	−0.16	0.02	−0.03	−0.08	−0.05	−0.26
17 *SALES_VOLATILITY*	0.14	−0.01	−0.05	0.04	−0.12	−0.08	0.06	0.03	−0.02	−0.19	−0.05	0.05
18 *CFO*	−0.16	−0.01	0.08	−0.01	0.21	0.13	−0.13	0.00	0.03	0.33	0.08	−0.54
19 *CFO_VOLATILITY*	0.33	−0.01	−0.12	0.01	−0.29	−0.16	−0.01	0.05	−0.06	−0.34	−0.18	0.23
20 *LAGGED_ACCRUALS*	−0.10	0.02	0.03	−0.04	0.07	0.05	0.03	−0.03	0.05	0.05	0.09	−0.17
21 *ISSUE*	0.01	0.00	0.06	0.03	0.13	0.05	−0.03	0.02	−0.03	0.17	0.00	−0.01
22 *LITRISK*	0.12	−0.02	−0.10	−0.01	−0.17	−0.09	−0.04	0.01	−0.07	−0.18	−0.10	0.16
23 *DELOITTE_CY*	−0.02	0.03	0.00	0.05	0.02	0.05	0.02	−0.03	0.06	0.02	0.03	−0.05
24 *POST*	0.05	−0.03	0.03	−0.01	0.05	−0.01	−0.06	0.00	0.01	0.09	−0.01	0.10

Panel E: Abnormal Accruals and Restatement Analysis (*GROWTH* to *POST*)

		13	14	15	16	17	18	19	20	21	22	23	24
13	*GROWTH*	1.00											
14	*MB*	0.24	1.00										

(continued on next page)

TABLE 4 (continued)

	13	14	15	16	17	18	19	20	21	22	23	24
15 *LEVERAGE*	−0.03	0.04	1.00									
16 *BANKRUPTCY*	0.28	0.21	−0.36	1.00								
17 *SALES_VOLATILITY*	0.09	0.00	−0.11	0.04	1.00							
18 *CFO*	0.23	0.03	0.00	0.31	0.01	1.00						
19 *CFO_VOLATILITY*	0.14	0.20	−0.18	−0.01	0.34	−0.27	1.00					
20 *LAGGED_ACCRUALS*	0.04	−0.09	−0.05	0.12	0.00	0.03	−0.10	1.00				
21 *ISSUE*	0.16	0.16	0.30	−0.10	−0.07	−0.06	0.00	−0.06	1.00			
22 *LITRISK*	0.01	0.11	−0.18	0.07	0.01	−0.18	0.22	−0.03	−0.04	1.00		
23 *DELOITTE_CY*	0.00	−0.05	0.02	−0.02	0.05	0.04	−0.04	0.04	−0.05	−0.06	1.00	
24 *POST*	−0.15	−0.19	0.06	−0.10	0.02	0.00	−0.01	−0.12	−0.05	0.01	−0.01	1.00

Correlations of 0.016, 0.020, and 0.026 (in absolute value) are significant at $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.
See Appendix A for variable definitions.

and *DELOITTE_PY* × *YR09*, both being positive and statistically significant ($p < 0.01$) in column (2).[17]

In both columns (1) and (2) of Table 5, Panel A, the coefficients on *DELOITTE_PY* are negative and statistically significant ($p < 0.05$), indicating that the likelihood of a client switch away from Deloitte was less than the likelihood of switches among the other Big 4. The coefficient on *POST* in column (1) is negative and marginally statistically significant ($p < 0.10$), suggesting that the likelihood of switches among the other Big 4 was lower during the 2008–2010 post-censure period than in the 2005–2007 pre-censure time period. Moreover, this effect appears concentrated in year 2009, where the coefficient on *YR09* is negative and significant at $p < 0.01$.

Turning next to client gains, Table 5, Panel A shows that the coefficients on *DELOITTE_CY* in both columns (3) and (4) are not statistically significant ($p > 0.10$), indicating that in the pre-event period 2005–2007, there is no significant difference in the likelihood of a company switching to Deloitte as compared to switches among the other Big 4. However, the coefficient on *DELOITTE_CY* × *POST* in column (3) is negative and marginally statistically significant ($p < 0.10$), indicating that the likelihood of a client switch to Deloitte relative to the likelihood of switches among the other Big 4 fell during the post-censure period. This reduction in the ability of Deloitte to gain clients during the post-event period spans the three years following the PCAOB sanction because the coefficients on the three interaction terms in column (4) are all negative and marginally statistically significant ($p < 0.10$).

Table 5, Panel B presents a series of coefficient restriction tests that analyze economic significance and highlight the difference-in-differences aspect of our research design. For brevity, we report results for coefficients in columns (1) and (3) only. The top one-third of the panel is for the 2005–2007 pre-event period, the middle one-third for the 2008–2010 post-event period, and the

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[17] Ai and Norton (2003) show that in a logit model with interaction terms, the effect of the interaction term on expected probability can be different in sign from the coefficient loading on the interaction term. However, Puhani (2012) extends Ai and Norton (2003) to show that when the interaction term is simply the product of a treatment group dummy variable (e.g., *DELOITTE_xx*) and a treatment period dummy variable (e.g., *POST*), the sign of the treatment effect is equal to the sign of the coefficient of the interaction term. Based on insights derived from Puhani (2012), we believe it is appropriate to infer the sign of the treatment effect based on the sign of the *DELOITTE_xx* × *POST* coefficient as we have done.

TABLE 5
Auditor Switch Analysis

Panel A: Model Estimates

	Exp. Sign	Parameter	Auditor Switch (Loss Sample and Specification)		Auditor Switch (Gain Sample and Specification)	
			(1)	(2)	(3)	(4)
Intercept	?	β_0	−4.757***	−4.742***	−3.923***	−3.894***
			(−8.28)	(−8.38)	(−5.24)	(−5.08)
GROWTH	+	β_1	0.058	0.025	0.140	0.119
			(0.31)	(0.12)	(0.95)	(0.75)
ABSDACC	+	β_2	3.270***	3.304***	2.654***	2.642**
			(2.98)	(2.90)	(2.58)	(2.51)
INVREC	+	β_3	0.441	0.393	0.631	0.593
			(0.71)	(0.61)	(1.30)	(1.19)
GC	+	β_4	0.366	0.372	0.062	0.065
			(0.59)	(0.60)	(0.11)	(0.12)
MODOP	+	β_5	0.200	0.144	0.099	0.056
			(1.26)	(0.80)	(0.72)	(0.34)
TENURE	?	β_6	−0.343***	−0.349***	−0.147	−0.150
			(−2.99)	(−2.98)	(−0.73)	(−0.74)
ROA	−	β_7	0.392	0.402	0.667	0.690
			(1.06)	(1.01)	(1.63)	(1.64)
LOSS	+	β_8	0.633***	0.662***	0.663***	0.692***
			(3.96)	(3.62)	(4.75)	(4.42)
LEVERAGE	+	β_9	0.407	0.385	0.173	0.153
			(0.98)	(0.91)	(0.41)	(0.36)
ΔLEVERAGE	+	β_{10}	0.118	−0.029	0.685	0.611
			(0.13)	(−0.03)	(0.89)	(0.74)
CASH	−	β_{11}	0.220	0.226	−0.323	−0.320
			(0.50)	(0.51)	(−0.64)	(−0.63)

(continued on next page)

TABLE 5 (continued)

	Exp. Sign	Parameter	Auditor Switch (Loss Sample and Specification) (1)	(2)	Auditor Switch (Gain Sample and Specification) (3)	(4)
MISMATCH	+	β_{12}	−0.901***	−0.907***	−0.339	−0.358
			(−7.62)	(−6.83)	(−0.86)	(−0.92)
SIZE	−	β_{13}	−0.111***	−0.106***	−0.151**	−0.150**
			(−3.55)	(−3.26)	(−2.36)	(−2.27)
ΔSIZE	+	β_{14}	0.596**	0.657**	0.324	0.387
			(2.12)	(2.20)	(1.25)	(1.43)
M_A	+	β_{15}	0.053	0.044	0.158	0.149
			(0.26)	(0.23)	(1.00)	(0.96)
ABNRML_FEE	+	β_{16}	0.711***	0.705***	0.578***	0.575***
			(3.79)	(3.75)	(4.19)	(4.23)
DELOITTE_PY	?	δ_1	−0.274**	−0.272**	0.499	0.500
			(−2.44)	(−2.42)	(0.34)	(0.34)
POST	?	δ_2	−0.443*		0.130	
			(−1.94)		(0.59)	
DELOITTE_PY × POST	?	δ_3	0.896***		−0.758**	
			(3.73)		(−2.17)	
YR08	?	δ_4		−0.241		0.266
				(−1.44)		(1.38)
YR09	?	δ_5		−0.460***		0.242
				(−11.47)		(0.79)
YR10	?	δ_6		−0.649		−0.146
				(−1.04)		(−0.42)
DELOITTE_PY × YR08	?	δ_7		0.917***		−0.846*
				(4.16)		(−1.81)
DELOITTE_PY × YR09	?	δ_8		1.111***		−0.690**
				(22.32)		(−1.99)
DELOITTE_PY × YR10	?	δ_9		0.443		−0.769*
				(0.78)		(−1.88)

(continued on next page)

TABLE 5 (continued)

	Exp. Sign	Parameter	Auditor Switch (Loss Sample and Specification) (1)	Auditor Switch (Loss Sample and Specification) (2)	Auditor Switch (Gain Sample and Specification) (3)	Auditor Switch (Gain Sample and Specification) (4)
n			9,970	9,970	10,013	10,013
Pseudo R^2			0.053	0.055	0.038	0.039

Panel B: Difference-in-Differences Analysis

		Loss Sample			Gain Sample		
Period	Description	Other Big 4	Deloitte	Diff.	Other Big 4	Deloitte	Diff.
2005–2007	Log-odds switching risk	−4.757***	−5.031***	−0.274**	−3.923***	−3.424***	0.499
	Measured by	β_0	$\beta_0 + \delta_1$	δ_1	β_0	$\beta_0 + \delta_1$	δ_1
2008–2010	Log-odds switching risk	−5.200***	−4.578***	0.622***	−3.793***	−4.052**	−0.259
	Measured by	$\beta_0 + \delta_2$	$\beta_0 + \delta_1 + \delta_2 + \delta_3$	$\delta_1 + \delta_3$	$\beta_0 + \delta_2$	$\beta_0 + \delta_1 + \delta_2 + \delta_3$	$\delta_1 + \delta_3$
Change	Log-odds switching risk	−0.443*	0.453***	0.896***	0.130	−0.628**	−0.758**
	Measured by	δ_2	$\delta_2 + \delta_3$	δ_3	δ_2	$\delta_2 + \delta_3$	δ_3

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*, **, *** Denote significance at $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

t-statistics are in parentheses.

The switching model is estimated by maximum likelihood as a logit regression. The dependent variable in the model is *SWITCH*, which equals 1 if the client switched auditors, and 0 otherwise. Standard errors control for clustering by auditor. Significance levels are based on two-tailed tests. The model includes industry dummy variables that allow the intercept to vary for each of the *j* two-digit SIC industry groups (β_{0j}).

The model in Panel A, column (1) is: $SWITCH_{i,t} = \beta_{0j} + \Sigma\beta_k CONTROL_{i,t-1} + \delta_1 DELOITTE\_PY_{i,t} + \delta_2 POST_{i,t} + \delta_3 DELOITTE\_PY_{i,t} \times POST_{i,t} + \varepsilon_{i,t}$.

The model in Panel A, column (2) is: $SWITCH_{i,t} = \beta_{0j} + \Sigma\beta_k CONTROL_{i,t-1} + \delta_1 DELOITTE\_PY_{i,t} + \Sigma\delta_k YRyy_{i,t} + \Sigma\delta_k DELOITTE\_PY_{i,t} \times YRyy_{i,t} + \varepsilon_{i,t}$.

The model in Panel A, column (3) is: $SWITCH_{i,t} = \beta_{0j} + \Sigma\beta_k CONTROL_{i,t-1} + \delta_1 DELOITTE\_CY_{i,t} + \delta_2 POST_{i,t} + \delta_3 DELOITTE\_CY_{i,t} \times POST_{i,t} + \varepsilon_{i,t}$.

The model in Panel A, column (4) is: $SWITCH_{i,t} = \beta_{0j} + \Sigma\beta_k CONTROL_{i,t-1} + \delta_1 DELOITTE\_CY_{i,t} + \Sigma\delta_k YRyy_{i,t} + \Sigma\delta_k DELOITTE\_CY_{i,t} \times YRyy_{i,t} + \varepsilon_{i,t}$.

All coefficients in Panel B refer to column 1 (3) in Table 5, Panel A, for the loss (gain) sample.

See Appendix A for other variable definitions.

bottom one-third reports tests of change between the two periods. Each of these one-third sections reports coefficient restriction functions that measure client change log-odds for Deloitte and the other Big 4.

Table 5, Panel B shows that client loss log-odds were lower for Deloitte (–0.274, $p < 0.05$) than that for the other Big 4 for the 2005–2007 period. In contrast, for the 2008–2010 period, client loss log-odds were higher for Deloitte (0.622, $p < 0.01$) than for the other Big 4. Comparing across periods, client loss log-odds declined marginally (–0.443, $p < 0.10$) for the other Big 4, while the client loss log-odds for Deloitte increased (0.453, $p < 0.01$). The difference-in-differences comparison reveals that Deloitte's pre/post change in client loss log-odds (0.453) exceeded that of the other Big 4 (–0.443) by 0.896 ($p < 0.01$).

Exponentiating the estimated coefficients converts the switching risk log-odds into a more easily interpreted odds ratio measure, which, in this instance, reflects the ratio of switching risk odds for Deloitte to the other Big 4. In the 2005–2007 pre-event period, the odds of a client switch away from Deloitte were only 76.0 percent of the switching risk odds among the other Big 4 ($e^{\delta 1} = 0.760$, $p < 0.05$). In the 2008–2010 post-event period, the odds of a switch away from Deloitte were 186 percent of the switching risk odds of the other Big 4 ($e^{\delta 1+\delta 3} = 1.86$, $p < 0.01$). Comparing the pre- and post-periods, the odds of a client switch among the other Big 4 declined marginally by 35.8 percent ($1 - e^{\delta 2} = 0.358$, $p < 0.10$), while the odds of a client switch away from Deloitte increased by 157 percent ($e^{\delta 2+\delta 3} = 1.57$, $p < 0.01$). Finally, the difference-in-differences odds ratio indicates that the odds of a switch away from Deloitte, relative to the switching risk odds among the other Big 4, grew by 245 percent between the pre- and post-censure periods ($2.45 = 1.86/0.760 = 1.57/[1 - 0.358] = e^{\delta 3}$, $p < 0.01$). That is, the PCAOB censure is associated with a 245 percent increase in the odds of Deloitte losing a client to another Big 4 firm during the three years following the December 2007 PCAOB censure.

Similar analyses for gains reveals in Table 5, Panel B that Deloitte's client gain log-odds were no different from that for the other Big 4 (0.499, $p > 0.10$) for the 2005–2007 period. The same is true for the 2008–2010 period (–0.259, $p > 0.10$). The difference-in-differences comparison of Deloitte's pre/post change to that of the other Big 4 shows that Deloitte's temporal change in log-odds of client gain (–0.628, $p < 0.05$) was smaller than that of the other Big 4 (0.130, $p > 0.10$), and the difference was significant (–0.758, $p < 0.05$). Exponentiating the value of δ_3 yields the difference-in-differences measure of switching risk odds for Deloitte relative to the other Big 4. It indicates that the odds of a switch to Deloitte, relative to the switching risk odds among the other Big 4, declined by 53.1 percent between the pre- and post-censure periods ($0.531 = 1 - e^{\delta 3}$, $p < 0.05$). These results suggest that Deloitte was less able to attract new clients following the December 2007 PCAOB censure.

Untabulated results using the propensity score matched sample method indicate that the coefficient on *DELOITTE_PY* × *POST* is 0.310 and marginally statistically significant ($p < 0.10$), suggesting that the likelihood of a client switch away from Deloitte, relative to the likelihood of switches among the other Big 4, widened in the post-censure period. This heightened switching risk of Deloitte during the post-event period was primarily concentrated in the second year following the PCAOB sanction. Moreover, untabulated results for client gains indicate that the reduction in the ability of Deloitte to gain clients during the post-event period was confined to year 2010, as indicated by the coefficient on *DELOITTE_PY* × *YR10* being negative and significant at $p < 0.05$. The collective evidence suggests our inferences are not materially affected when we utilize the propensity score to control for differences in client characteristics between Deloitte and other Big 4 firms while estimating the treatment effect.

Overall, the results shown in Table 5 reject H1 and indicate that Deloitte experienced a higher likelihood of losing an existing client to other Big 4 firms and a lower likelihood of gaining a new client relative to other Big 4 firms following the PCAOB censure. These results are consistent with the notion that the 2007 PCAOB disciplinary order against Deloitte imposed actual costs on Deloitte by adversely affecting its switching risk.

Change in Audit Fee Analysis

Regression results for two alternative specifications of Model (3) are reported in Table 6, Panel A. The specification in column (1) constrains the post-event change to be the same for each of the years 2008–2010, and the column (2) specification allows the post-event change to differ over the three post-event years. Two points with respect to Model (3) are noteworthy. First, because the model is estimated from a sample consisting of continuing clients who retained the same Big 4 auditor in each of the two years used to calculate the year-to-year change variables, the model examines the changes in audit pricing on continuing clients. Second, because the dependent variable Δ*AUDIT_FEE* is specified in natural log form, it can be interpreted as the annual within-firm rate of growth in audit fees.

In both specifications in Table 6, Panel A the control variables are generally significant with signs consistent with prior research and in accordance with economic intuition. We note that our control for fee pressure, *ABNRML_FEE*, loads with a negative and strongly significant coefficient, which suggests that companies with above (below) normal audit fees tend to receive fee reductions (increases) the following year. That is, audit fees exhibit temporal mean reversion during our sample period. To the extent that *ABNRML_FEE* is an effective control for mean reversion, we can be confident that any temporal changes in audit fee changes that are captured by the test variables are not due to systematic overpricing or underpricing by Deloitte during 2005–2007 that resolves during 2008–2010.

The coefficients δ_1–δ_3 in column (1) of Table 6, Panel A, and the coefficients δ_1 and δ_4–δ_9 in column (2) are of interest. In column (1), the coefficient on *DELOITTE_CY* is positive and statistically significant ($p < 0.01$), indicating that during 2005–2007, Deloitte enjoyed fee growth rates larger than the other Big 4. The coefficient on *POST* in column (1) is negative and statistically significant ($p < 0.01$), suggesting that fee growth rates declined among the other Big 4 during the 2008–2010 post-censure period. Moreover, this effect was present in each of the three post-censure years, as reflected in the coefficient on each *YRyy* variable being negative and significant at $p < 0.01$. The coefficient on *DELOITTE_CY* × *POST* in column (1) is negative and statistically significant ($p < 0.05$), indicating that Deloitte fee growth rates, relative to the other Big 4, declined in the post-censure period. This decline in Deloitte fee growth rates during the post-event period was primarily concentrated in the second and third years following the PCAOB sanction, with coefficients on *DELOITTE_CY* × *YR09* and *DELOITTE_CY* × *YR10* being both negative and statistically significant ($p < 0.01$) in column (2).

Panel B of Table 6 presents a series of coefficient restriction tests that analyze economic significance and highlight the difference-in-differences aspect of our research design. Inspection of Panel B shows that during the pre-event period 2005–2007, other Big 4 auditors increased audit fees on continuing clients by 10.7 percent (0.107, $p < 0.10$), Deloitte increased audit fees by 14.3 percent (0.143, $p < 0.05$), and the 3.6 percent difference (i.e., 3.6 percent = 14.3 percent – 10.7 percent) in audit fee changes was statistically significant ($p < 0.01$). In the 2008–2010 post-event period, other Big 4 auditors reduced audit fees by 0.3 percent ($p > 0.10$), while Deloitte increased audit fees by 0.9 percent ($p > 0.10$), and the 1.2 percent difference in audit fee changes was insignificant (0.012, $p > 0.10$). Comparing the two periods, annual audit fee changes declined by 11 percent ($p < 0.01$) for the other Big 4, 13.4 percent for Deloitte (–0.134, $p < 0.01$), and the difference was statistically significant (–0.024, $p < 0.05$). Results show that Deloitte moved from imposing audit fee increases that were 3.6

TABLE 6
Change in Audit Fee Analysis

Panel A: Model Estimates

	Exp. Sign	Parameter	(1)	(2)
Intercept	?	β_0	0.107*	0.107*
			(2.86)	(2.91)
$\Delta SIZE$	+	β_1	0.301***	0.301***
			(13.16)	(13.35)
$\Delta ln(SEGMENTS)$	+	β_2	0.063***	0.062***
			(15.26)	(14.29)
$\Delta CATA$	+	β_3	−0.019	−0.018
			(−0.42)	(−0.41)
$\Delta FOREIGN$	+	β_4	0.110	0.107
			(2.21)	(2.18)
$\Delta QUICK$	−	β_5	−0.014***	−0.013***
			(−5.95)	(−6.06)
$\Delta LEVERAGE$	+	β_6	0.089***	0.078***
			(19.65)	(18.79)
ΔROA	−	β_7	−0.123**	−0.131**
			(−3.86)	(−4.99)
$\Delta LOSS$	+	β_8	0.052***	0.050***
			(7.64)	(7.46)
ΔGC	+	β_9	0.080**	0.072*
			(3.45)	(3.01)
$\Delta MODOP$	+	β_{10}	0.006*	0.005
			(2.85)	(2.01)
ABNRML_FEE	−	β_{11}	−0.244***	−0.244***
			(−21.97)	(−21.79)
DELOITTE_CY	?	δ_1	0.036***	0.036***
			(8.53)	(8.57)
POST	?	δ_2	−0.110***	
			(−30.56)	
DELOITTE_CY × *POST*	?	δ_3	−0.024**	
			(−5.46)	
YR08	?	δ_4		−0.093***
				(−32.18)
YR09	?	δ_5		−0.135***
				(−21.86)
YR10	?	δ_6		−0.103***
				(−13.49)
DELOITTE_CY × *YR08*	?	δ_7		0.003
				(0.61)
DELOITTE_CY × *YR09*	?	δ_8		−0.032**
				(−4.25)
DELOITTE_CY × *YR10*	?	δ_9		−0.044***
				(−6.67)
n			9,766	9,766
R^2			0.258	0.260

(continued on next page)

TABLE 6 (continued)

Panel B: Difference-in-Differences Analysis

Period	Description	Other Big 4	Deloitte	Difference
2005–2007	Change in logged audit fee	0.107*	0.143**	0.036***
	Measured by	β_0	$\beta_0 + \delta_1$	δ_1
2008–2010	Change in logged audit fee	−0.003	0.009	0.012
	Measured by	$\beta_0 + \delta_2$	$\beta_0 + \delta_1 + \delta_2 + \delta_3$	$\delta_1 + \delta_3$
Change	Change in logged audit fee	−0.110***	−0.134***	−0.024**
	Measured by	δ_2	$\delta_2 + \delta_3$	δ_3

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*, **, *** Denote significance at $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.
t-statistics are in parentheses.
The model is estimated as an ordinary least squares regression. The dependent variable in the model is $\Delta AUDIT\_FEE$, which is the annual change in the natural log of audit fees. Δ represents a change in a variable relative to its value in the previous year. Standard errors control for clustering by auditor. Significance levels are based on two-tailed tests. The model includes industry dummy variables that allow the intercept to vary for each of the *j* two-digit SIC industry groups (β_{0j}).
The model in column (1) is: $\Delta AUDIT\_FEE_{i,t} = \beta_{0j} + \Sigma\beta_k CONTROL_{i,t} + \delta_1 DELOITTE\_CY_{i,t} + \delta_2 POST_{i,t} + \delta_3 DELOITTE\_CY_{i,t} \times POST_{i,t} + \varepsilon_{i,t}$.
The model in column (2) is: $\Delta AUDIT\_FEE_{i,t} = \beta_{0j} + \Sigma\beta_k CONTROL_{i,t} + \delta_1 DELOITTE\_CY_{i,t} + \Sigma\delta_k YRyy_{i,t} + \Sigma\delta_k DELOITTE\_CY_{i,t} \times YRyy_{i,t} + \varepsilon_{i,t}$.
All coefficients in Panel B refer to Specification 1 in Table 6, Panel A.
See Appendix A for other variable definitions.

percentage points larger than the other Big 4 during 2005–2007 ($\delta_1 = 0.036$, $p < 0.01$) to a near-zero fee growth rate during 2008–2010 that was insignificantly different from the near-zero fee growth rates manifested by the other Big 4 ($\delta_1 + \delta_3 = 0.012$, $p > 0.10$), for a statistically significant 2.4 percent decline in fee growth rate. This 2.4 percent decline represents our estimate of the PCAOB censure effect on Deloitte's pricing on continuing audit engagements. In other words, we estimate that the PCAOB censure was associated with a 2.4 percent incremental decrease in Deloitte's rate of audit fee changes charged to continuing clients relative to other Big 4 auditors. Untabulated results using the propensity score matched sample method show a 3.5 percent decline in Deloitte audit fee growth rates of ($\delta_3 = -0.035$, $p < 0.01$), which confirms the main results discussed above. Overall, the results are consistent with the notion that Deloitte curtailed fee growth rates in an attempt to retain its audit clients following the damage to its reputation.

Audit Quality Analysis

Regression results for alternative specifications of Model (4) are reported in Table 7. Columns (1) and (2) present the test of H3 based on OLS regression analysis of the dependent variable *ABSDACC* (i.e., client's level of absolute abnormal accruals). Columns (3) and (4) present the test of H3 based on logistic regression analysis of the dependent variable *RESTATE* (i.e., 1/0 indicator variable showing whether the client subsequently restated the financial statements due to an accounting application error). All models are significant ($p < 0.01$) with R^2 (Pseudo-R^2) of the OLS (logit) models around 0.18 (0.07). The coefficients on auditor- and client-level control variables are generally consistent with predictions. In terms of H3, the

TABLE 7

Abnormal Accrual and Restatement Analysis

	Exp. Sign	Parameter	DV = *ABSDACC*		DV = *RESTATE*	
			(1)	(2)	(3)	(4)
Intercept	?	β_0	−0.037	−0.040	−12.967***	−13.055***
			(−1.39)	(−1.40)	(−2.85)	(−2.80)
INFLUENCE	?	β_1	0.006	0.006	−0.424	−0.418
			(0.64)	(0.69)	(−0.26)	(−0.25)
SHORT	+	β_2	0.003	0.004	−0.180	−0.178
			(0.88)	(0.96)	(−1.01)	(−1.03)
AUDIT_FEE	+	β_3	0.005*	0.005*	0.170	0.174
			(3.08)	(2.64)	(0.33)	(0.33)
NONAUDIT_FEE	+	β_4	−0.000	−0.000	0.003	0.002
			(−0.61)	(−0.64)	(0.22)	(0.20)
EFFORT	−	β_5	0.000	−0.000	−0.001	−0.001
			(0.22)	(−0.16)	(−0.11)	(−0.13)
SEC_REG_OFF	+	β_6	0.004	0.003	−0.225	−0.228
			(1.47)	(1.39)	(−0.62)	(−0.63)
LNPOP	−	β_7	0.003	0.003	−0.175	−0.177
			(1.96)	(1.98)	(−0.94)	(−0.92)
SIZE	−	β_8	−0.008***	−0.008***	−0.021	−0.021
			(−31.76)	(−35.63)	(−0.08)	(−0.08)
SEGMENTS	?	β_9	0.001***	0.001***	−0.085	−0.084
			(7.25)	(7.32)	(−1.21)	(−1.21)
LOSS	−	β_{10}	0.026***	0.026***	0.295	0.303
			(10.54)	(9.97)	(1.45)	(1.47)
GROWTH	+	β_{11}	0.029***	0.030***	0.147	0.154
			(6.20)	(6.31)	(0.30)	(0.30)
MB	+	β_{12}	0.002	0.003*	−0.145***	−0.136**
			(1.76)	(2.71)	(−2.78)	(−2.32)
LEVERAGE	+	β_{13}	0.004	0.001	−0.117	−0.132
			(0.64)	(0.19)	(−0.21)	(−0.25)
BANKRUPTCY	−	β_{14}	−0.001***	−0.001***	−0.030***	−0.031***
			(−12.38)	(−12.59)	(−3.08)	(−3.14)
SALES_VOLATILITY	+	β_{15}	0.005	0.007	−0.320	−0.298
			(0.61)	(0.85)	(−0.78)	(−0.70)
CFO	−	β_{16}	0.032*	0.029	0.287	0.287
			(2.39)	(2.20)	(0.84)	(0.73)
CFO_VOLATILITY	+	β_{17}	0.198***	0.193***	1.101	1.043
			(8.50)	(8.27)	(0.89)	(0.84)
LAGGED_ACCRUALS	−	β_{18}	−0.010	−0.014	0.913*	0.836*
			(−0.67)	(−0.96)	(1.83)	(1.68)
ISSUE	+	β_{19}	0.002	0.002	0.122***	0.120***
			(0.69)	(0.78)	(2.80)	(2.74)
LITRISK	+	β_{20}	−0.002	−0.002	−0.646***	−0.651***
			(−0.89)	(−0.98)	(−2.88)	(−2.93)
DELOITTE_CY	?	δ_1	0.001	0.001	0.287	0.288
			(1.28)	(1.22)	(1.16)	(1.17)

(continued on next page)

TABLE 7 (continued)

			DV = *ABSDACC*		DV = *RESTATE*	
	Exp. Sign	**Parameter**	**(1)**	**(2)**	**(3)**	**(4)**
POST	?	δ_2	0.007**		−0.268***	
			(3.34)		(−3.51)	
DELOITTE_CY × *POST*	?	δ_3	−0.002		0.025	
			(−1.44)		(0.19)	
YR08	?	δ_4		0.018**		−0.183
				(5.56)		(−1.46)
YR09	?	δ_5		0.001		−0.442
				(0.57)		(−1.39)
YR10	?	δ_6		0.002		−0.195
				(1.85)		(−1.03)
DELOITTE_CY × *YR08*	?	δ_7		−0.005		0.095
				(−1.98)		(0.70)
DELOITTE_CY × *YR09*	?	δ_8		0.002		0.145
				(1.14)		(0.43)
DELOITTE_CY × *YR10*	?	δ_9		−0.005		−0.201
				(−0.59)		(−0.92)
n			5,709	5,709	5,709	5,709
R^2			0.183	0.189	0.071	0.072

*, **, *** Denote significance at $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

t-statistics are in parentheses.

The model in column (1) is: $ABSDACC_{i,t} = \beta_{0j} + \Sigma\beta_k CONTROL_{i,t} + \delta_1 DELOITTE\_CY_{i,t} + \delta_2 POST_{i,t} + \delta_3 DELOITTE\_CY_{i,t} \times POST_{i,t} + \varepsilon_{i,t}$.

The model in column (2) is: $ABSDACC_{i,t} = \beta_{0j} + \Sigma\beta_k CONTROL_{i,t} + \delta_1 DELOITTE\_CY_{i,t} + \Sigma\delta_k YRyy_{i,t} + \Sigma\delta_k DELOITTE\_CY_{i,t} \times YRyy_{i,t} + \varepsilon_{i,t}$.

Both accrual models are estimated as ordinary least squares regressions. The dependent variable in the model is *ABSDACC*, which is the absolute value of abnormal accruals. See Appendix A for other variable definitions. Standard errors control for clustering by auditor. Significance levels are based on two-tailed tests. The model includes industry dummy variables that allow the intercept to vary for each of the *j* two-digit SIC industry groups (β_{0j}).

The model in column (3) is: $RESTATE_{i,t} = \beta_{0j} + \Sigma\beta_k CONTROL_{i,t} + \delta_1 DELOITTE\_CY_{i,t} + \delta_2 POST_{i,t} + \delta_3 DELOITTE\_CY_{i,t} \times POST_{i,t} + \varepsilon_{i,t}$.

The model in column (4) is: $RESTATE_{i,t} = \beta_{0j} + \Sigma\beta_k CONTROL_{i,t} + \delta_1 DELOITTE\_CY_{i,t} + \Sigma\delta_k YRyy_{i,t} + \Sigma\delta_k DELOITTE\_CY_{i,t} \times YRyy_{i,t} + \varepsilon_{i,t}$.

Both restatement models are estimated by maximum likelihood as logit regressions. The dependent variable in the model is *RESTATE*, which equals 1 if the client subsequently restates the current-year financial statements due to an accounting rule application error (excluding clerical errors), and 0 otherwise. Standard errors control for clustering by auditor. Significance levels are based on two-tailed tests. The model includes industry dummy variables that allow the intercept to vary for each of the *j* two-digit SIC industry groups (β_{0j}).

See Appendix A for other variable definitions.

coefficients on *DELOITTE_CY* × *POST* and *DELOITTE_CY* × *YRyy* are of interest. None of these coefficients are statistically significant ($p > 0.10$). The results reported in Table 7 provide no evidence to suggest that Deloitte's audit quality was different from that of the other Big 4 auditors in the post-PCAOB censure period.[18]

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[18] In additional analysis, we also examined the propensity to issue going concern opinions (for distressed clients) as a proxy for audit quality, and untabulated results for the going concern analysis are consistent with the "no results" finding of the tabulated abnormal accruals and restatement analysis.

V. CONCLUDING REMARKS

DeFond (2010) notes that one channel by which PCAOB inspections can improve audit quality is to provide auditors' *ex ante* incentives by having tighter standards and imposing harsh sanctions for failure to comply. Our study examines (1) the nature of the actual costs imposed on Deloitte by the PCAOB's first-ever censure of a Big 4 audit firm in December 2007, and (2) the impact of the censure on Deloitte's audit quality.

Our results indicate that the 2007 PCAOB disciplinary order against Deloitte imposed significant actual costs on the firm, as measured by a subsequent change in Deloitte's switching risk in the form of an increase in the firm's existing client loss rate to other Big 4 firms and a drop in the client gain rate. Our results also suggest that Deloitte reduced the rate at which they were escalating fees over a three-year period following the censure to stem the tide of client defections to other Big 4 auditors. Thus, our findings suggest that PCAOB sanctions can inflict actual harm on a Big 4 auditor by affecting audit committee perceptions of the firm's audit quality. Overall, our results suggest a role for non-litigation factors, such as auditor reputation loss, via PCAOB oversight in the market for Big 4 audits in the U.S.

Finally, we are unable to detect a difference between Deloitte's audit quality and that of the other Big 4 auditors during a three-year window either before or after the PCAOB censure. Our finding is consistent with the view expressed in the prior literature (Lennox and Pittman 2010) that PCAOB oversight during the time period of our study was focused on documentation and substantiation compliance rather than on a holistic assessment of Big 4 audit outcomes. Alternatively, the extant research methodologies we use may have been unable to detect the improvement in Deloitte's audit quality. Hence, a fuller assessment of the benefits of PCAOB oversight may have to await the development of newer, more powerful methodologies for assessing audit quality.

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APPENDIX A

Definition of Variables

Variable	Description
Dependent Variables	
SWITCH	1 if client changed auditor between prior ($t-1$) and current (t) year, and 0 otherwise.
ΔAUDIT_FEE	Year-to-year change in the natural log of audit fees (in dollars).
ABSDACC	Absolute value of abnormal accruals, where abnormal accruals are obtained from the modified Jones (1991) model estimated by year and two-digit SIC code using those industries with at least 15 annual observations. Consistent with Kothari et al (2005), the model includes return on assets as a control for firm performance.
RESTATE	1 if the client subsequently restates the current-year financial statements due to an accounting rule application error (excluding clerical errors), 0 otherwise (i.e., Audit Analytics variables *RES_ACCOUNTING* = 1 and *RES_CLER_ERR* = 0 in WRDS).
Test Variables	
DELOITTE_CY	1 if Deloitte served as auditor in the fiscal year, 0 otherwise.
DELOITTE_PY	1 if Deloitte served as auditor in the prior fiscal year, 0 otherwise.
POST	1 if the fiscal period ends between the period June 1, 2008–May 31, 2011, and 0 otherwise.
YR08	1 if the fiscal period ends between the period June 1, 2008–May 31, 2009, and 0 otherwise.
YR09	1 if the fiscal period ends between the period June 1, 2009–May 31, 2010, and 0 otherwise.
YR10	1 if the fiscal period ends between the period June 1, 2010–May 31, 2011, and 0 otherwise.
Other Variables	
ABNRML_FEE	Abnormal audit fee, measured as the residual from audit fee model (Equation (3)) estimated in level (not change) form without the Deloitte main and interactive test variables (i.e., $AUDIT\_FEE_{i,t} = \beta_{0j} + \Sigma\beta_k\ CONTROL_{i,t} + \varepsilon_{i,t}$).
AUDIT_FEE	Natural log of annual audit fees paid to the company's auditor.
BANKRUPTCY	Altman's z-score.
CASH	Total cash divided by total assets.
CATA	Ratio of current assets to total assets.
CFO	Operating cash flows deflated by lagged total assets.
CFO_VOLATILITY	Standard deviation of *CFO* over the current and preceding two years.
EFFORT	Audit fees deflated by the square root of total assets.
FOREIGN	Proportion of total sales from foreign operations.
GC	1 if the client received a going concern audit opinion in year t, and 0 otherwise.
GROWTH	Ending total assets less beginning total assets, with the difference deflated by beginning total assets.
INFLUENCE	Ratio of the client's annual fees for all services to the sum of annual fees for all clients of the engagement office.

(continued on next page)

APPENDIX A (continued)

Variable	Description
INVREC	Inventory plus receivables divided by total assets.
ISSUE	1 if external financing (stock and long-term debt issues) during the current and following fiscal year exceeds 5 percent of current-year ending total assets, and 0 otherwise.
LAGGED_ACCRUALS	Prior-year asset-deflated total accruals.
LEVERAGE	Ratio of debt to total assets.
LITRISK	1 if the firm's SIC code is within one of the following SIC groups: 2833–2836, 3570–3577, 3600–3674, 5200–5961, or 7370–7374, and 0 otherwise.
LNPOP	Natural log of the population of the city in which the auditor is located. Population totals are obtained from the U.S. Census Bureau.
LOSS	1 if $ROA < 0$, and 0 otherwise.
M_A	1 if the client was involved in a merger or acquisition in the current or preceding year, and 0 otherwise.
MB	Natural log of the ratio of the market to book value of equity.
MISMATCH	1 if the Shu (2000) methodology identifies a client/auditor mismatch, and 0 otherwise. See Appendix B for details.
MODOP	1 if the firm received a modified audit opinion for anything other than going concern, and 0 otherwise.
NONAUDIT_FEE	Natural log of annual nonaudit fees paid to the company's auditor.
QUICK	Ratio of current assets (excluding inventories) to current liabilities.
ROA	Net income before extraordinary items divided by average total assets.
SALES_VOLATILITY	Standard deviation of sales revenue (deflated by lagged assets) over the current and preceding two years.
SEC_REG_OFF	1 if the auditor's office is not in the same city as an SEC Regional Office, and 0 otherwise.
SEGMENTS	Number of operating segments reported in Compustat for the fiscal year.
SHORT	1 if auditor tenure is three years or less, and 0 otherwise.
SIZE	Natural log of total assets (in millions of $).
TENURE	Number of years the incumbent auditor has served as the client's auditor.

APPENDIX B

Details on Construction of *Mismatch*

Consistent with Shu (2000) and Landsman et al. (2009), we first estimate by logit the following auditor choice model. The model is estimated separately for each fiscal year:

$$Prob(BIG4_{it} = 1) = F(\beta_0 + \beta_1 SIZE_{it} + \beta_2 ACQUISITION_{it} + \beta_3 EXFINANCE_{it} + \beta_4 PROFITABILITY_{it} + \beta_5 MtoB_{it}),$$

where:

$BIG4 = 1$ if the company used a Big 4 auditor, 0 otherwise;
$SIZE =$ natural log of total assets;
$ACQUISITION =$ investing cash outflows from acquisitions;
$EXFINANCE =$ asset-deflated sum of long-term debt issuances and stock issuances;

PROFITABILITY = asset-deflated income before extraordinary items; and
MtoB = market value of equity deflated by book value of common equity.

Next, we use the estimated coefficients to calculate the probability that a firm-year will use a Big 4 auditor. This predicted probability is compared to a cutoff probability threshold. Observations with a predicted probability greater (less) than the cutoff probability are predicted (not) to use a Big 4 auditor. A client/auditor mismatch occurs when a company predicted to use a Big 4 auditor uses a non-Big 4 auditor, or *vice versa*.

The cutoff probability threshold, which varies by year, is the cutoff value that minimizes the sample classification errors in that year.

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Executive Compensation and Regulation-Imposed Governance: Evidence from the California Nonprofit Integrity Act of 2004

Sandip Dhole
The University of Melbourne

Saleha B. Khumawala
University of Houston

Sagarika Mishra
Deakin University

Tharindra Ranasinghe
Singapore Management University

ABSTRACT: This study examines the impact of the California Nonprofit Integrity Act of 2004 on CEO compensation costs in affected organizations. Contrary to the stated objective of the Act that executive compensation is "just and reasonable," we find that CEO compensation costs for affected nonprofits during the post-regulation periods have increased by about 6.3 percent when compared with a control group of comparable unaffected nonprofits. In addition, the relative increase in CEO compensation appears to come from nonprofits that have experienced greater regulatory cost increases. We do not find evidence that the Act resulted in a change in CEO pay performance sensitivity. The observed CEO pay increase is not systematically different across nonprofits that underpaid versus those that overpaid their CEOs during pre-Act periods. Overall, this paper highlights the unintended consequences of regulatory attempts to enhance governance in the not-for-profit sector.

Keywords: *executive compensation; governance; regulation; nonprofits; California Nonprofit Integrity Act of 2004.*

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I. INTRODUCTION

This paper investigates the impact of California's Nonprofit Integrity Act of 2004 on the executive compensation costs of affected nonprofit organizations. The California Nonprofit Integrity Act of 2004 (hereafter, "the Act" or "the regulation") became effective on January 1, 2005 and is closely modeled after the Sarbanes-Oxley Act (hereafter, SOX).[1] It is regarded as a pioneering piece of regulatory legislation attempting to strengthen governance mechanisms in the not-for-profit sector (Ljung 2005). The Act includes requirements for boards of directors of nonprofit organizations to approve the compensation levels of their Chief Executive Officer (CEO) and Chief Financial Officer (CFO) or treasurer and to ensure that the compensation paid is "just and reasonable."

Both the press and regulators have continuously raised concerns over excess executive compensation in nonprofits (e.g., see Gosselin and Zitner 1997; Lublin 2003; Walsh 1996). The Internal Revenue Service (IRS) introduced sanctions in Internal Revenue Code (IRC) Section 4958 to curb excess compensation in nonprofits, as a part of the Taxpayer Bill of Rights 2 (1996).[2] Further, in June 2004 the U.S. Senate Finance Committee held a hearing titled, "Charity Oversight and Reform: Keeping Bad Things from Happening to Good Charities" (U.S. Senate Finance Committee 2004). The Commissioner of the Internal Revenue Service, Mark W. Everson (2004), testified that:

> We need go no further than our daily newspapers to learn that some charities and private foundations have their own governance problems . . . We are concerned that the governing boards of tax-exempt organizations are not, in all cases, exercising sufficient diligence as they set compensation for the leadership of the organizations. There have been numerous recent reports of executives of both private foundations and public charities who are receiving unreasonably large compensation packages.

Nonprofit managers operate with less compensation oversight than their corporate counterparts. Without shareholders and with less fear of takeovers, nonprofit managers avoid some of the most important disciplining mechanisms facing private sector executives. The numerous reports in the popular press of gross governance failures and scandals in the not-for-profit sector appear consistent with this notion and provide a rationale for regulatory interventions (Ljung 2005).[3] Responding to concerns of excess executive compensation, the California Nonprofit Integrity Act of 2004 requires boards of nonprofits to review and approve CEO and CFO compensation levels as "just and reasonable." Beyond such explicit provisions, other provisions of the Act can also have executive compensation implications, as governance and managerial compensation are extensively interlinked (Hermalin 2005).

Some academics and practitioners have argued that these regulations are either ineffective, or that the costs associated with compliance exceed the intended benefits (Gilkeson 2007). Moreover, they contend that governance mechanisms exogenously imposed by regulators are inherently undesirable in the not-for-profit sector, as the most efficient methods to address agency problems evolve endogenously (Fama and Jensen 1983a, 1983b). Mulligan (2007) and Brakman-Reiser (2004) contend that nonprofit regulations inspired by the Sarbanes-Oxley Act (SOX) of 2002 are likely to be ineffective as they do not adjust for key differences between for-profit and not-for-profit sectors.

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[1] Jackson (2006) terms the Act as "California Sarbanes-Oxley clone legislation."

[2] However, the effectiveness of Section 4958 is debated (see Frumkin 2001).

[3] For example, see Lewis (2000), Harris (2002), Whoriskey and Salmon (2003), Healy (2004), Herbert (2006), McWhirter (2011), and Flaherty and Stephens (2013).

Despite the wider academic debate on the relative merits of nonprofit regulations, there is a lack of empirical research on this issue in general, and on the attempts to control executive compensation in particular. We contribute to this debate by examining the executive compensation implications of the Act. Specifically, we analyze whether the Act has affected the executive compensation levels in affected nonprofits and, if so, how these effects have occurred.

If the salary levels of affected executives were, in fact, unreasonably large and the provisions of the Act aimed at curbing them are effective, then we should observe a relative reduction in post-regulation compensation costs.[4] However, the Act could also lead to higher executive pay for several reasons. First, Hermalin (2005) demonstrates that stronger governance can lead to higher executive pay, as more diligent boards induce greater effort from the managers, for which they should be compensated. Second, as opponents of regulations point out, if the exogenously imposed governance mechanisms are inefficient and result in deadweight costs, then some of these costs are likely to take the form of higher executive compensation because executives must be compensated for the added work caused by increased regulation. Third, Nagel (2007) highlights that greater awareness of salary practices brought about by attempts to improve governance could lead to an increase in salaries of executives who are being underpaid, with no accompanying reduction in salaries of overpaid executives, thereby resulting in an overall increase in executive compensation.

Using a sample of affected California nonprofits from 2000–2010, we first analyze the pre- to post-regulation differences in CEO compensation levels. As our control sample, we use a group of similar nonprofits from states other than California. We use a difference-in-differences research design in order to minimize the likelihood that the observed effects could be due to other unrelated omitted factors. Contrary to the Act's intentions, our findings indicate that the Act has resulted in an *increase* in CEO compensation levels for the affected California nonprofits, compared to the control group. This increase is approximately 6.3 percent higher than the corresponding pre- to post-regulation change in CEO compensation of similar nonprofits in other states.

Next, we investigate probable explanations for this observation of higher post-Act CEO pay for affected nonprofits. We rule out that our finding is due to the introduction of more efficient compensation contracts that minimize agency costs in post-Act periods, because we do not find a post-Act improvement in pay performance sensitivity for affected nonprofits.[5] To the extent that the requirements of the Act led to regulatory costs, in terms of incremental reporting and administrative burdens for which the CEO should be compensated, the higher post-Act CEO compensation would be more pronounced for those nonprofits that likely faced greater regulatory costs. Using the change in accounting fees as the proxy for regulatory costs induced by the Act (Neely 2011), we find this indeed to be the case. Higher post-Act CEO compensation is primarily for those nonprofits that likely experienced greater regulatory costs due to the Act. Finally, we also examine whether higher pay is an unintended outcome of compensation benchmarking where previously underpaid CEOs experience salary increases, while salaries of previously overpaid CEOs remain relatively stable (Nagel 2007). However, we do not find the impact of the Act on executive compensation to differ statistically between entities that likely underpaid and overpaid their CEOs in the pre-Act periods. Overall, added administrative burden on the CEO emerges as the most credible explanation for the higher post-Act compensation.

In additional tests, we investigate whether the Act has had a disproportionate impact on relatively smaller nonprofits, because administrative and reporting requirements of regulations of this nature are argued to be especially burdensome to smaller entities (Gilkeson 2007). Consistent with this argument, we find some support indicating that the increase in post-Act CEO compensation occurred primarily in smaller affected nonprofits.

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[4] The Act should lead to a *relative* reduction in compensation. That is, the post-Act growth rate of CEO compensation for affected nonprofits should be lower than that for unaffected nonprofits in order for the Act's provisions to meet their overall objectives.

[5] Using program ratio as the performance measure, we also examine whether the Act has led to improved performance. We fail to find any evidence that it did. We discuss this result in Section V.

This paper makes important contributions to the literature on compensation regulation in general and compensation regulation in the not-for-profit sector in particular. While important institutional differences make it difficult to extend findings on compensation regulation in the corporate sector into nonprofit settings, the empirical results remain largely mixed even within the corporate sector.[6] Evidence on how regulatory actions affect executive compensation in the not-for-profit sector is especially scarce. In a study addressing the financial reporting quality and commercial fundraising activity implications of the Act, Neely (2011) documents that the post-Act increase in compensation of officers and directors is smaller for affected nonprofits when compared with those that are unaffected. Unlike Neely (2011), who investigates total compensation of managers and directors, we focus squarely on CEO compensation, as the provisions of Section 12586(g) of the Act that attempt to ensure that executive compensation is "just and reasonable" apply only to the CEO and CFO. We find that the CEO pay has increased more for affected nonprofits in post-Act periods. Moreover, we also investigate potential explanations for this finding and conclude that added administrative burdens are a likely cause. In this regard, this paper expands our knowledge of the efficacy of governance regulations in the not-for-profit sector. The findings of the paper could be especially important for policy makers as more states contemplate similar SOX-inspired nonprofit legislation (Mulligan 2007).

The remainder of the paper proceeds as follows: In Section II, we discuss the related literature and build our main hypothesis. Section III presents the sample selection and research design, descriptive statistics, and our main findings. In Section IV, we explore the potential explanations for our finding of higher post-Act CEO pay for affected nonprofits. Section V reports the results of some additional tests, and Section VI concludes.

II. LITERATURE REVIEW AND HYPOTHESIS DEVELOPMENT

Whether regulation-imposed governance mechanisms lead to more desirable executive compensation practices is a contentious issue in both the corporate and the not-for-profit sectors. For instance, the evidence on the efficacy of the Internal Revenue Code Section 162(m), which aimed to improve executive compensation practices in the corporate sector by curtailing the corporate tax deductibility of nonperformance-related compensation for top executives, is largely mixed. While Perry and Zenner (2001) and Balsam and Ryan (2007) report that the provision has been somewhat successful in moderating the growth rate of executive compensation, Rose and Wolfram (2002) fail to find convincing evidence of the regulation improving pay performance sensitivity. Further, Balsam (2002) and Lublin (2003) report that there has been a substantial increase in CEO compensation subsequent to the enactment of Section 162(m). Similarly, Nagel (2007) investigates executive compensation effects of the mandated disclosure of CEO pay in 1992 and the 2004 New York Stock Exchange (NYSE) listing requirement that compensation committees be independent and fully responsible for hiring and paying the CEOs, and fails to find evidence to suggest that either regulation is successful in curtailing excess CEO pay.

It could be argued that there is a comparatively greater role for regulatory intervention in governance and executive compensation practices in the not-for-profit sector because the absence of residual claimants with strong monitoring incentives and the virtual immunity from ousters via takeovers present nonprofit managers with greater latitude to expropriate the firm's assets and engage in other forms of opportunistic behavior, such as abusive compensation practices (e.g., see

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[6] We discuss these prior findings in the corporate sector in some detail in Section II.

Barringer 1992; DiLorenzo 1992; Gaul and Borowski 1993; Gosselin and Zitner 1997; Lublin 2003; Walsh 1996). That said, Fama and Jensen (1983a, 1983b) argue that absence of alienable residual claims in the not-for-profit sector is simply a natural response to avoid the donor-residual claimant agency problems that could arise in such entities. They note that due to this unique feature, nonprofits have developed some governance mechanisms that are distinctly different from those of for-profit corporations. These include self-perpetuating boards, presence of major donors on the boards of directors, and the general absence of internal agents as voting members of the board. If the agency conflicts in nonprofits can be efficiently resolved through governance mechanisms that emerge endogenously, then the regulatory imposition of governance rules would be both unnecessary and costly. The doubts raised by Mulligan (2007) and Brakman-Reiser (2004) about the potential efficacy of SOX-inspired governance reforms in the not-for-profit sector are consistent with such reasoning.

California's Nonprofit Integrity Act of 2004 is considered a watershed event in attempts to regulate governance and compensation practices in the not-for-profit sector with SOX-like legislation (Ljung 2005). The Act broadly covers two areas: (1) governance, and (2) commercial fundraising activities of nonprofit organizations. The main provisions include:

1. Requirement to prepare publicly accessible annual financial statements audited by an independent public accountant (CPA) (Government Code Section 12586(e)(1)).
2. Establishment of an audit committee that is responsible for making recommendations on the hiring and firing of auditors, negotiating auditor compensation, approving nonaudit services by the auditor, and ensuring that financial affairs of the nonprofit organization are in order (Government Code Section 12586(e)(2)).
3. Requirement that nonprofits have their governing boards or authorized board committees review and approve the compensation of the Chief Executive Officer (CEO) or President, and the compensation of the Chief Financial Officer (CFO) or treasurer, and ensure that the payment is "just and reasonable" (Government Code Section 12586(g)).
4. Numerous provisions aimed at regulating commercial fundraising activities by nonprofit organizations (Government Code Section 12599).

The Act applies to all charitable organizations, unincorporated associations, and trusts over which the State of California or the Attorney General has enforcement or supervisory powers. However, provisions 1 and 2 above apply only to those nonprofits with gross revenues exceeding \$2 million. The \$2 million threshold excludes grants received from governmental entities, if the nonprofit provides an accounting of how it used the grant funds. Moreover, educational institutions, religious organizations, hospitals, licensed healthcare service plans, and cemeteries are exempt from the provisions of the Act.

This study focuses on executive compensation implications of the Act. We note that despite the inconclusive evidence on the impact of regulation on compensation practices described above, there is a paucity of research examining this issue in the not-for-profit sector in general, and in the context of the Act in particular. One exception is Neely (2011), who examines whether the Act had any impact on executive compensation. However, our paper differs substantially from Neely's (2011) study of the initial impact of the Act along a number of key dimensions. While Neely (2011) examines the pre- to post-Act differences in executive compensation within his broader objective of investigating reporting quality and fundraising issues, our study is exclusively focused on the executive compensation implications of the Act. Therefore, we investigate not only whether the Act has impacted executive compensation of affected entities, but also, more importantly, potential explanations for such impact. Moreover, while Neely (2011) inspects the changes in compensation of officers, directors, etc., as reported in line 25 of Form 990, we focus our study on CEO compensation because the provisions of Section 12586(g) of the Act that attempt to ensure executive compensation is "just and reasonable" apply only to the CEO and CFO.[7]

In a normative sense, if the executive compensation in nonprofits is indeed excessive and the compensation review requirements of the Act are effective, then we would expect the Act to temper the growth in CEO compensation of affected nonprofits, when compared with nonprofits unaffected by the Act. In other words, pre- to post-Act increases in CEO compensation levels would be smaller for affected nonprofits, *relative to* nonprofits that are unaffected by the Act. *Ceteris paribus,* such an observation will be broadly consistent with the normative notion of the Act being successful in addressing the problem of excess compensation in California nonprofits.

However, there are also reasons to believe that the provisions of the Act would not make any meaningful impact on executive compensation. For instance, one could argue that the compensation-related provisions of the Act lack the teeth to make a substantial difference, because the Act neither defines what is "just and reasonable" compensation, nor does it prescribe any penalties to boards that fail to appropriately "review and approve" executive compensation as prescribed by the Act. In addition, Bebchuk and Fried (2003) argue in the context of the corporate sector that ensuring efficient executive compensation through board monitoring is exceedingly difficult because board members have strong incentives to maintain cordial relations with management. If this characterization is accurate for the not-for-profit sector as well, then the requirement of the Act that the board "review and approve" CEO and CFO compensation may not serve as a sufficiently strong preventive measure.

Finally, it is also possible for the provisions of the Act to result in a relative *increase* in executive compensation levels. This could take place due to at least three reasons. First, Hermalin (2005) demonstrates analytically that stronger governance can lead to higher CEO pay as more diligent boards make the CEO work harder in equilibrium, so that the CEO's equilibrium utility falls, resulting in him or her demanding to be compensated for this loss of utility. Second, according to those who argue that most efficient governance mechanisms emerge endogenously (e.g., Fama and Jensen 1983a, 1983b), attempts at exogenously imposing governance mechanisms are inherently value-destroying. Under this view, existing levels of executive compensation can be seen as the most efficient equilibrium for the purpose of attracting and retaining managerial talent and minimizing agency conflicts. Added reporting and administrative burdens imposed by regulations increase the level of non-value-adding activities carried out by both the organization as a whole and its executives in particular, thereby creating deadweight costs. A third reason is the possibility that the regulation simply increases the opportunistic use of compensation benchmarking practices where salaries of CEOs who are overpaid remain unchanged, while those of CEOs who are underpaid are increased (Nagel 2007). Of the three potential reasons that may result in higher executive pay, the first cannot be viewed as an adverse outcome, as higher pay is associated with greater managerial effort. The second reason suggests a failure of the regulation in the form of an unintended negative consequence. The third reason can be viewed as an unintended consequence in that the aggregate executive compensation costs increase without an accompanying improvement in performance.

As the above discussion postulates, the impact of the California Nonprofit Integrity Act of 2004 on executive compensation levels is best determined through empirical examination. Hence, we present our main hypothesis as follows (null form):

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[7] Additionally, while Neely's (2011) sample is limited to only one year before and after the enactment of the Act, we carry out our study over a longer sample period, thus allowing us a longer time horizon to evaluate the effectiveness of the Act. Also, while Neely's (2011) analysis is primarily univariate, we employ multivariate model specifications with a comprehensive set of control variables so that the concerns over confounding effects are greatly mitigated.

H1: In comparison to a control group of nonprofits, the nonprofits affected by the California Nonprofit Integrity Act of 2004 do not experience a pre- to post-regulation difference in the level of CEO compensation.

III. SAMPLE SELECTION, RESEARCH DESIGN, DESCRIPTIVE STATISTICS, AND MAIN RESULTS

Sample Selection

We obtain the Statistics of Income (SOI) and Compensation (SOI Comp) data files from the National Center for Charitable Statistics (NCCS). We investigate how the Act has influenced the CEO compensation of affected California nonprofits in comparison to a control group of unaffected nonprofits domiciled in other states. The control group consists of nonprofits that meet the industry and size requirements of the California Nonprofit Integrity Act and, hence, would have been covered by the Act had they been domiciled in California. We exclude nonprofits domiciled in the states of Connecticut, Kansas, Maine, Massachusetts, New Hampshire, and West Virginia from the control group because these states have also enacted nonprofit governance regulations during the sample period (Mulligan 2007), following in the footsteps of California. Because both the treatment group and the control group are exposed to identical regulations at the federal level, any results we find cannot be attributed to regulatory and oversight changes at the federal level, such as the Internal Revenue Service's Intermediate Sanctions (Internal Revenue Code Section 4958) issued in January 2002.

Our sample covers the period from 2000 to 2010.[8] We remove observations from the implementation year (2004) from our analyses. We also ensure that the reporting year covered in each observation falls entirely within either pre- or post-Act periods. Firm-years that end on or before December 31, 2003 are designated as pre-Act, while those that begin on or after January 1, 2005 are designated as post-Act.

We start with an initial sample of 166,345 firm-year observations for the 2000–2010 period on the NCCS database. We delete observations from industries that are exempted from the requirements of the Act, observations with missing values for key variables, and observations from Connecticut, Kansas, Maine, Massachusetts, New Hampshire, and West Virginia, as these states enacted similar legislation following California. This leads to a loss of 91,273 firm-year observations. A further 25,907 observations are lost because they had missing values for CEO compensation. We also delete observations from organizations with gross annual receipts of less than $2 million, because certain important provisions of the Act do not apply to them. We lose 15,435 firm-year observations as a result. We ensure that a firm has at least one observation each in pre- and post-Act periods. This inclusion restriction causes us to lose 13,907 firm-year observations. The sample is further truncated at 1 percent and 99 percent of the distributions of the dependent and independent variables to mitigate the effects of possible outliers. This causes us to lose a further 991 firm-year observations. Thus, our final sample consists of 18,832 firm-year observations (5,466 unique firms), of which affected California nonprofits account for 2,205 firm-year observations (639 unique firms). The control group consists of 16,627 firm-year observations (4,827 unique firms).

Table 1 presents the industry distribution of our sample. There is a significant concentration of organizations in Human Services, which accounts for 30.1 percent and 36.5 percent of the

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[8] The length of our sample period is largely consistent with those of compensation regulation studies in the corporate sector (e.g., see Balsam and Ryan 2007; Rose and Wolfram 2002). Use of a relatively long event window increases statistical power, but the likelihood of confounding events affecting the results increases. We mitigate the latter concern by using difference-in-differences specifications in all our tests.

TABLE 1

Industry Distribution by Number of Observations and Percentage

	California		**Control**	
Industry	**Obs.**	**%**	**Obs.**	**%**
Arts, Culture, and Humanities	341	16.0	2,118	12.7
Environment	96	4.4	461	2.8
Animal-Related	53	2.4	396	2.4
Voluntary Health Associations and Medical Disciplines	38	2.0	428	2.6
Medical Research	83	3.8	550	3.3
Employment	74	3.4	504	3.0
Food, Agriculture, and Nutrition	36	1.6	161	1.0
Housing and Shelter	252	11.4	745	4.5
Public Safety, Disaster Preparedness and Relief	4	0.2	40	0.2
Recreation and Sports	41	1.9	384	2.3
Youth Development	49	2.2	372	2.2
Human Services	682	30.1	6,063	36.5
International, Foreign Affairs, and National Security	56	2.5	711	4.3
Civil Rights, Social Action and Advocacy	8	0.4	93	0.6
Community Improvement and Capacity Building	51	2.3	501	3.0
Philanthropy, Voluntarism, and Grantmaking Foundations	216	9.8	2,171	13.1
Science and Technology	45	2.0	446	2.7
Social Science	33	1.5	126	0.8
Public and Societal Benefit	29	1.3	250	1.5
Mutual and Membership Benefit	18	0.8	107	0.6
Total	2,205	100.00	16,627	100.00

The sample consists of 501(c)(3) organizations that report to the Attorney General's office in California and the other U.S. states, *excluding* Connecticut, Kansas, Maine, Massachusetts, New Hampshire, and West Virginia. We exclude these states because they have enacted similar legislations, following in the footsteps of California. The IRS Code Section 501(c)(3) provides for an exemption from federal income tax and allows donors to these organizations to deduct their donation on their federal income tax return. To qualify for 501(c)(3) exemption, an organization must be organized to operate exclusively for one or more of the following purposes: charitable, religious, educational, scientific, literary, testing for public safety, fostering national or international amateur sports competition, and/or the prevention of cruelty to children or animals. The sample covers the period from 2000 to 2010, excluding 2004, the implementation year of the California Nonprofit Integrity Act. The sample nonprofits have data on both pre- and post-Act periods. The sample excludes nonprofits with gross receipts less than $2 million because some of the Act's provisions do not apply to them. Nonprofits classified as religious, grantmaking, health, education, or cemeteries are excluded as they are exempt from the Act.

California and control samples, respectively. Arts, Culture, and Humanities organizations and Philanthropy, Voluntarism, and Grantmaking Foundations account for approximately 12–15 percent and 10–13 percent of the observations across the groups, respectively. While we do not find substantial differences in industry distribution between treatment and control groups, we control for industry membership in all our empirical models.

Research Design

H1 tests whether the Act has had an impact on the executive compensation costs of affected nonprofits. We estimate the following model to test H1 (for firm *i* and year *t*) while clustering the standard errors at the firm and year levels (Petersen 2009):

$$\begin{aligned}LnComp_{it} = {} & \alpha_0 + \alpha_1 Calif_i + \alpha_2 Post_t + \alpha_3 Calif_i{*}Post_t + \alpha_4 LnTA_{it} + \alpha_5 Complex_{it} + \alpha_6 Endow_{it} \\ & + \alpha_7 Donations_{it} + \alpha_8 Excess_{it} + \alpha_9 StateInc_{it} + \alpha_{10} StateGov_{it} + \alpha_{11} Program_{it} \\ & + \alpha_j Industry_i + \varepsilon_{it}.\end{aligned} \tag{1}$$

The natural logarithm (log) of CEO compensation (*LnComp*) is the dependent variable. The SOI Comp database reports compensation paid to officers, directors, and key employees. Assuming that the CEO is the highest-paid executive, we measure CEO compensation as the sum of compensation (C020), contributions to employment benefit plans and deferred compensation (C030), and expense accounts and other allowances (C040).

Calif is an indicator variable taking the value of 1 if the organization is based in California, and 0 otherwise, while the indicator variable *Post* takes the value of 1 for years after the Act (2005–2010), and 0 otherwise (2000–2003). The variable of interest is the interaction term, *Calif* $*$ *Post,* with the estimated coefficient, α_3, indicating the pre- to post-Act difference in CEO compensation levels of affected California nonprofits, in comparison to the control group. This difference-in-differences design alleviates concerns of omitted correlated variables. A negative (positive) and significant α_3 would indicate that the Act has resulted in a relative reduction (increase) in CEO compensation of affected nonprofits.

Our regression model includes a number of control variables that are potentially associated with executive compensation. We use the log of total assets (*LnTA*) from line 59 of Form 990 to control for entity size because CEOs of larger nonprofits likely receive higher compensation (Hallock 2002). Conjecturing a positive association between organization complexity and executive compensation, we control for complexity (*Complex*), measured as the number of revenue sources (lines 1 through 11 of Form 990). We control for endowment size (*Endow*) because prior literature suggests that it is associated with CEO compensation (Core, Guay, and Verdi 2006). Endowment size is measured as the sum of cash, savings, and investment securities (line 45, column (b) + line 46, column (b) + line 54, column (b) of Form 990), deflated by total expenses from line 17 of Form 990. We control for total donations from line 1d of Form 990, scaled by total revenue from line 12 of Form 990 (*Donations*). Since size is already controlled for, we do not make a directional prediction on the sign of *Donations*. We also control for the excess of revenues over expenses (*Excess*) (line 12 of Form 990 – line 17 of Form 990). The relation between *Excess* and *LnComp* would be negative if lower levels of revenues over expenses are symptomatic of excessive executive compensation. On the other hand, a positive relation would be expected if highly paid CEOs attempt to create larger *Excess* in order to secure their future benefits or to create an impression of better expense management.

Executive compensation levels across states are likely to be affected by state-level differences in broad economic factors. We use log of per capita income of the state (*StateInc*) to control for such effects. We obtain these data from the website of the Bureau of Economic Analysis.[9] To control for differences in state level factors that could affect the governance of nonprofits independent of the Act, we employ a state level governance index (*StateGov*) based on the 17-factor index developed by Desai and Yetman (2006), who include 11 detection factors and six prosecution factors. Accordingly, *StateGov* could vary from 1 to 17 depending on the number of governance measures present in each state. We control for the program ratio (*Program*) based on Baber, Daniel, and Roberts (2002), who find that changes in executive compensation are associated with changes in the program ratio. The program ratio is measured as the ratio of program-related expenses (line 13 of Form 990) to total expenses (line 17 of Form 990). We control for industry-level differences in CEO compensation through industry dummy variables (*Industry*) defined in terms of the National Taxonomy of Exempt Entities (NTEE1) codes.

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[9] See Report SA1-3 at: http://www.bea.gov/iTable/iTable.cfm?ReqID=70&step=1&isuri=1&acrdn=4

Descriptive Statistics

Table 2 presents descriptive statistics for California (Panel A) and the control sample (Panel B), respectively. Each panel depicts descriptive statistics pertaining to the entire group, as well as separately for the pre- and post-Act subgroups. The mean (median) CEO compensation for the California and control samples are 0.253 (0.175), and 0.224 (0.165) million dollars, respectively. Untabulated analyses indicate that the mean of CEO compensation, as well as means of *Total Assets, Total Revenue, Total Expenses, Donations, Endow,* and *Excess,* are larger for California than for the control group. Higher CEO compensation for the California nonprofits is consistent with these organizations being generally larger along several dimensions. The control group exhibits a marginally higher program ratio. The two groups are not statistically different in terms of per capita income.

In untabulated tests, we also examine differences between the pre- and post-Act subgroups. *Compensation* and the other financial variables are higher in post-Act periods for both the California and control groups. In the post-Act period, mean CEO compensation increased by \$99,000 for California nonprofits and \$76,000 for control group nonprofits. This difference-in-differences univariate compensation is statistically significant (untabulated $p < 0.001$), indicating a greater post-Act relative CEO compensation growth for California. Next, we report the results of multivariate tests that control for potential confounding factors.

Main Results

Table 3 reports estimation results for tests of H1, where we investigate the pre- to post-Act differences in CEO compensation levels. The coefficient of interest is α_3, the coefficient on the interaction term *Calif* $*$ *Post*. It is positive and significant ($\alpha_3 = 0.063$, $p = 0.040$), indicating that the CEO salaries of affected California nonprofits have increased more post-Act than the control group. Because the dependent variable is defined as the log of CEO compensation, the *Calif* $*$ *Post* interaction can be interpreted in percentage terms (Wooldridge 2009). This indicates that the pre- to post-Act increase in the CEO compensation of affected California nonprofits is approximately 6.3 percent higher than the corresponding change in CEO compensation of similar nonprofits in other states. The relative increase is economically significant in percentage terms and, given the pre-Act mean annual compensation of approximately \$253,000, translates to an increase of about \$16,000. Thus, the results suggest that the Act has had an unintended consequence of raising CEO pay in affected California nonprofits, relative to the control sample.

In terms of the control variables, in Table 3, the coefficient α_1 on *Calif,* which captures the difference in CEO pay levels for California versus the control group in pre-Act periods, is not significant ($\alpha_1 = -0.013$, $p = 0.659$). This suggests that although salaries of California nonprofit CEOs were higher during the pre-Act period, as indicated in the descriptive statistics, these pre-Act differences can be explained by differences in other control variable determinants of pay. As expected, CEO pay is positively associated with size (*LnTA*) and organizational complexity (*Complex*). The relation between CEO pay and total endowment (*Endow*) and excess (*Excess*) is insignificant.[10] While there is a negative relation between compensation and donations (*Donations*), a positive association is found between per capita state income (*StateInc*) and the dependent

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[10] Core et al. (2006) report a positive relation between executive compensation and excess endowment. However, in Core at al. (2006), excess endowment is measured as the residual from an expectations model where total endowment is regressed on a number of explanatory variables. Here, we simply control for the level of endowment.

TABLE 2

Descriptive Statistics

Panel A: California

	Total (n = 2,205)			Pre-Act (n = 731)			Post-Act (n = 1,474)		
Variable	**Mean**	**Median**	**S. D.**	**Mean**	**Median**	**S. D.**	**Mean**	**Median**	**S. D.**
Compensation	0.253	0.175	0.357	0.187	0.147	0.161	0.286	0.193	0.418
Total Assets	87.463	32.136	149.622	58.862	22.366	100.807	101.647	37.599	166.893
Total Revenue	41.578	12.739	98.448	30.743	10.957	63.013	46.951	13.722	111.566
Total Expenses	33.921	10.130	84.439	25.886	8.566	59.195	37.905	10.829	94.254
Program	0.811	0.845	0.140	0.807	0.847	0.148	0.813	0.842	0.136
Donations	6.906	0.000	28.284	5.575	0.000	19.810	7.566	0.000	31.642
Endow	33.714	6.210	78.044	26.495	5.259	60.394	37.294	6.853	85.248
Excess	4.073	0.485	29.300	2.369	0.348	11.973	4.928	0.540	34.867
Complex	6.272	6.000	2.307	6.508	6.000	2.340	6.125	6.000	2.274
StateInc	0.039	0.039	0.004	0.035	0.034	0.001	0.041	0.042	0.003
StateGov	14.000	14.000	—	14.000	14.000	—	14.000	14.000	—

Panel B: Control Group

	Total (n = 16,627)			Pre-Act (n = 5,192)			Post-Act (n = 11,435)		
Variable	**Mean**	**Median**	**S. D.**	**Mean**	**Median**	**S. D.**	**Mean**	**Median**	**S. D.**
Compensation	0.224	0.165	0.197	0.172	0.134	0.126	0.248	0.183	0.217
Total Assets	74.591	31.599	131.859	52.140	21.143	99.615	84.785	37.780	142.982
Total Revenue	37.749	13.233	145.206	26.554	10.305	95.200	42.832	15.032	162.671
Total Expenses	31.701	10.348	132.692	22.177	8.182	85.352	36.026	11.649	149.116
Program	0.819	0.849	0.129	0.809	0.846	0.143	0.824	0.850	0.122
Donations	6.828	0.000	75.007	4.545	0.000	35.928	7.864	0.000	87.128
Endow	30.409	6.061	72.639	25.526	5.530	63.075	32.626	6.315	76.489
Excess	2.805	0.415	32.604	1.817	0.283	18.749	3.272	0.497	37.408
Complex	6.186	6.000	2.197	6.349	6.000	2.245	6.091	6.000	2.163
StateInc	0.038	0.036	0.007	0.033	0.032	0.005	0.039	0.037	0.007
StateGov	11.327	13.000	3.025	11.291	13.000	3.037	11.355	13.000	3.015

Panels A and B of Table 2 report descriptive statistics for the California nonprofits affected by Nonprofit Integrity Act (2004) and the control group respectively. The sample is based on the observations for the period 2000–2010, excluding 2004, as defined in Table 1.

All figures, other than *Endow, Program,* and *StateGov,* are expressed in US$ million.

Variable Definitions:

Compensation = total compensation paid to the CEO, from the NCCS Statistics of Income Compensation database, calculated as the sum of compensation (C020), contributions to employment benefit plans and deferred compensation (C030), and expense accounts and other allowances (C040);

Total Assets = total assets at the end of the year, line 59 on Form 990;

Total Revenue = total revenue, line item 12 on Form 990;

Total Expenses = total expenses, line item 17 on Form 990;

Program = ratio of program related expenses to total expenses (line 13, divided by line 17 on Form 990);

Donations = total donations, line item 1d on Form 990;

Endow = average of the sum of cash, savings, and investment securities (Line 45, column (b) + Line 46, column (b) + Line 54, column (b)) deflated by total expenses (Line 17) on Form 990;

Excess = excess of revenues over expenses (Line 12 – Line 17 on Form 990;

Complexity = number of revenue sources (lines 1 through 11 on Form 990);

StateInc = per capita income of the state as reported by the Bureau of Economic Analysis; and

StateGov = state-level (pre-Act) governance index measured as the number of governance factors present, as per Desai and Yetman (2006).

TABLE 3

Tests of H1
Impact of the California Nonprofit Integrity Act of 2004 on CEO Compensation

$$
\begin{aligned}
LnComp_{it} = {} & \alpha_0 + \alpha_1 Calif_i + \alpha_2 Post_t + \alpha_3 Calif_i{*}Post_t + \alpha_4 LnTA_{it} + \alpha_5 Complex_{it} + \alpha_6 Endow_{it} \\
& + \alpha_7 Donations_{it} + \alpha_8 Excess_{it} + \alpha_9 StateInc_{it} + \alpha_{10} StateGov_{it} + \alpha_{11} Program_{it} \\
& + \alpha_j Industry_i + \varepsilon_{it}.
\end{aligned}
$$

		Coefficient	p-value
Intercept	α_0	1.408**	0.024
Calif	α_1	−0.013	0.659
Post	α_2	0.068	0.248
*Calif * Post*	α_3	0.063**	0.040
LnTA	α_4	0.045**	0.024
Complex	α_5	0.023***	0.001
Endow	α_6	∼−0.000	0.562
Donations	α_7	−0.089***	0.008
Excess	α_8	0.007	0.220
StateInc	α_9	0.470***	<0.001
StateGov	α_{10}	0.005*	0.097
Program	α_{11}	−0.974***	<0.001
Industry Controls?		Yes	
Adj. R^2		0.434	
n		18,832	

***, **, * Indicate that coefficients are statistically different from 0 at the 1 percent, 5 percent, and 10 percent levels, respectively (two-tailed tests).

The sample is based on the observations for the period 2000–2010, excluding 2004, as defined in Table 1. Industry controls are employed through dummy variables defined in terms of one character National Taxonomy of Exempt Entities (NTEE1) Codes. ∼+ (∼−) indicate coefficients that are positive (negative), but approximate to 0 when rounded to three decimal places.

Variable Definitions:

LnComp = log of total compensation paid to the CEO. CEO compensation is from the NCCS Statistics of Income Compensation database, calculated as the sum of compensation (C020), contributions to employment benefit plans and deferred compensation (C030), and expense accounts and other allowances (C040);
Calif = a dummy variable that takes a value of 1 if the observation is from California, and 0 otherwise;
Post = a dummy variable that takes a value of 1 for years on or after 2004, and 0 otherwise;
LnTA = log of total assets at the end of the year. Total assets are from line 59 on Form 990;
Complex = number of revenue sources for the organization, based on lines 1 through 11 on Form 990;
Endow = average of the sum of cash, savings, and investment securities (Line 45, column (b) + Line 46, column (b) + Line 54, column (b)) deflated by total expenses (Line 17 on Form 990);
Donations = total donations, line item 1d on Form 990, scaled by total revenue (line 12 on Form 990);
Excess = natural logarithm of the excess of revenues over expenses, Line 12 − Line 17 on Form 990;
StateInc = log of per capita income of the state, as reported by the Bureau of Economic Analysis;
StateGov = state-level (pre-Act) governance index measured as the number of governance factors present, as per Desai and Yetman (2006); and
Program = ratio of program-related expenses to total expenses (line 13 divided by line 17 on Form 990).

variable, indicating that CEOs of higher-income states are paid more. The association between state-level governance (*StateGov*) and compensation is positive, consistent with a positive association between executive compensation and more demanding governance regimes. Finally, we observe a negative relation between CEO pay and program ratio (*Program*). While somewhat surprising, this relation is consistent with the findings of Core (2002) in the corporate sector, where relation between pay and accounting performance turns negative when the two variables are measured as levels.

IV. INVESTIGATION OF CAUSES BEHIND THE OBSERVED CHANGE IN POST-ACT CEO COMPENSATION

Results reported in the previous section indicate that the Act has resulted in relatively higher post-Act CEO compensation for affected California nonprofits. As briefly mentioned before, the post-Act compensation could be higher due to:

1. Stronger governance mechanisms introduced by the Act, resulting in more efficient compensation contracts where the CEO is induced to work harder and improve productive output, for which she or he is compensated with higher pay.
2. Deadweight regulatory costs as a result of more onerous reporting and administrative burdens of the Act.
3. Increased use of benchmarking practices in post-Act periods, resulting in previously underpaid executives receiving higher pay, without an accompanying decrease in pay for previously overpaid employees.

In this section, we investigate each of these explanations in detail to ascertain potential reasons behind the observed higher pay. Such an investigation is crucial for a comprehensive understanding of the executive compensation implications of the Act.

Impact of the California Nonprofit Integrity Act of 2004 on Pay Performance Sensitivity

While higher post-Act CEO pay may initially appear inconsistent with regulators' objective of making compensation more "just and reasonable," it could be argued that this outcome is, in fact, a result of more efficient compensation contracts induced by the Act.[11] The provision of the Act that requires the board to closely monitor CEO and CFO pay, and other provisions aimed at improving governance, may improve the process of determining executive pay (such as closer scrutiny by directors and the compensation committee, use of formal benchmarking, etc.) and strengthen the link between pay and performance, even though the total pay increases in terms of the dollar amount. In fact, Hermalin (2005) argues that executive pay may increase under a regime of improved governance, as managers are incentivized to work harder. An obvious way to achieve such incentive alignment would be to better align manager pay with organizational performance (Hölmstrom 1979). As long as the increased managerial effort leads to higher output for the firm, the resulting higher executive compensation may not be viewed as a negative outcome. Therefore, we investigate whether the Act has improved the relationship between executive compensation and firm performance (pay performance sensitivity).[12]

In the corporate sector, pay performance sensitivity is broadly measured as the relation between executive pay and a performance measure (see Murphy 1999, Equation (3)) with performance measures typically including either market returns or accounting measures (Lambert and Larcker 1987; Murphy 1985).[13] Program ratio, which captures the proportion of total expenses spent on program-related activities, is the most widely used performance measure in the not-for-profit sector (Baber et al. 2002). Prominent charity-rating organizations, such as the American Institute of Philanthropy's Charity Watch and Charity Navigator, use the program ratio as a primary input in their rating processes.[14] Therefore, we measure pay performance sensitivity as the relation between executive compensation and the program ratio (Baber et al. 2002; Krishnan, M. Yetman, and R. Yetman 2006).[15] Accordingly, we augment Model (1) by interacting the program ratio variable (*Program*) with dummy variables of *Post* and *Calif*:[16]

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[11] Here, as in the corporate sector literature, "efficient" compensation contracts are defined as compensation arrangements that minimize agency costs (Core, Guay, and Larcker 2003). Generally speaking, a stronger link between managerial compensation and output is associated with lower agency costs. (For instance, agency conflicts are fully eliminated in the extreme case of the owner-manager, where the entire output belongs to the manager.)

[12] In additional tests, we also investigate the impact of the Act on performance itself. See Section V.

[13] Jensen and Murphy (1990) define pay performance sensitivity as the dollar change in CEOs' wealth associated with a dollar change in the wealth of shareholders. We do not employ this definition as nonprofits are not traded in capital markets.

$$\begin{aligned} LnComp_{it} = {} & \beta_0 + \beta_1 Calif_i + \beta_2 Post_t + \beta_3 Calif_i{*}Post_t + \beta_4 LnTA_{it} + \beta_5 Complex_{it} \\ & + \beta_6 Endow_{it} + \beta_7 Donations_{it} + \beta_8 Excess_{it} + \beta_9 StateInc_{it} + \beta_{10} StateGov_{it} \\ & + \beta_{11} Program_{it} + \beta_{12} Program_{it}{*}Calif_i + \beta_{13} Program_{it}{*}Post_t \\ & + \beta_{14} Program_{it}{*}Calif_i{*}Post_t + \beta_j Industry_i + \varepsilon_{it}. \end{aligned} \tag{2}$$

The coefficient of interest is that of the three-way interaction term *Program* $*$ *Calif* $*$ *Post* (β_{14}). A positive and significant coefficient β_{14} would indicate that the CEO pay performance sensitivity has increased post-Act, suggesting compensation arrangements that are more closely aligned with organizational performance.

While Model (2) is a direct modification of Model (1), which was used in testing our main hypothesis, Core (2002) argues that pay performance sensitivity models are better specified when both compensation and performance are measured as change variables (see, also, Baber et al. 2002). Therefore, we also employ an additional regression specification where *LnComp* and *Program* (along with relevant interaction terms) are replaced by $\Delta LnComp$ and $\Delta Program$, respectively.

The results for Model (2) are reported in Column 1 of Table 4. The coefficient of interest is β_{14} on the three-way interaction term *Program* $*$ *Calif* $*$ *Post*. We find that β_{14} is not statistically significant ($\beta_{14} = 0.356$, $p = 0.192$). Hence, we do not find empirical evidence to support the notion that higher post-Act CEO pay is due to the Act invoking more efficient compensation contracts. We also note that while the coefficient on the interaction term *Calif* $*$ *Post* (β_3) is not significant in Column 1 of Table 4, the combined coefficient of *Calif* $*$ *Post* and *Program* $*$ *Calif* $*$ *Post* ($\beta_3 + \beta_{14}$) is positive and highly significant (untabulated, $\beta_3 + \beta_{14} = 0.130$, $p = 0.007$). Thus, for the average California program ratio of 81.1 percent, these coefficient estimates imply a relative post-Act pay increase of 6.3 percent, consistent with the previous inferences with respect to H1 from Model (1).[17]

The results from the alternate specification, where CEO compensation and program ratio are defined as change variables, are reported in Column 2 of Table 4 and they lead to very similar inferences. The coefficient on the three-way interaction term Δ*Program* $*$ *Calif* $*$ *Post* remains insignificant ($\beta_{14} = 0.412$, $p = 0.239$) and fails to support the notion of the Act improving pay performance sensitivity of affected nonprofits.

Impact of the California Nonprofit Integrity Act of 2004 on Executive Compensation due to Costs of Regulation

It is possible that the administrative burdens of the Act have increased the workload of the CEO in domains that are not directly related to entities' productive output (charitable activities). For

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[14] For further details on rating methodologies of Charity Guide and Charity Navigator, respectively, see: http://www.charitywatch.org/criteria.html/ and http://www.charitynavigator.org/index.cfm?bay=content.view&cpid=1284

[15] Baber et al. (2002) dissect program spending into a program ratio component and a revenue component. For parsimony, we include program ratio as the only performance measure. Incorporating revenue and the related interaction terms into the regression model does not alter our inferences.

[16] Although we measure pay performance sensitivity as the contemporaneous relation between performance and pay, use of lagged performance does not alter our inferences with respect to H2.

[17] $\beta_3 + 0.811 * \beta_{14} = -0.226 + 0.811 * 0.356 = 0.063$.

TABLE 4

Impact of the California Nonprofit Integrity Act of 2004 on Pay Performance Sensitivity

$$\begin{aligned} LnComp_{it}/\Delta LnComp_{it} = {} & \beta_0 + \beta_1 Calif_i + \beta_2 Post_t + \beta_3 Calif_i{*}Post_t + \beta_4 LnTA_{it} + \beta_5 Complex_{it} \\ & + \beta_6 Endow_{it} + \beta_7 Donations_{it} + \beta_8 Excess_{it} + \beta_9 StateInc_{it} \\ & + \beta_{10} StateGov_{it} + \beta_{11} Program_{it}/\Delta Program_{it} \\ & + \beta_{12} Program_{it}{*}Calif_i/\Delta Program_{it}{*}Calif_i \\ & + \beta_{13} Program_{it}{*}Post_t/\Delta Program_{it}{*}Post_t \\ & + \beta_{14} Program_{it}{*}Calif_i{*}Post_t/\Delta Program_{it}{*}Calif_i{*}Post_t \\ & + \beta_j Industry_i + \varepsilon_{it}. \end{aligned}$$

		Dependent Variable			
		LnComp		*ΔLnComp*	
Variable		**Coefficient**	**p-value**	**Coefficient**	**p-value**
Intercept	β_0	1.312**	0.038	−0.601**	0.016
Calif	β_1	0.185	0.297	0.001	0.965
Post	β_2	0.248*	0.091	0.033	0.209
*Calif * Post*	β_3	−0.226	0.219	−0.004	0.856
LnTA	β_4	0.0445**	0.024	∼−0.000	0.998
Complex	β_5	0.023***	0.001	−0.003	0.126
Endow	β_6	∼−0.000	0.550	∼−0.000**	0.026
Donations	β_7	−0.088***	0.009	−0.021	0.190
Excess	β_8	0.007	0.213	0.006**	0.028
StateInc	β_9	0.468***	<0.001	0.045**	0.039
StateGov	β_{10}	0.005	0.101	∼−0.000	0.632
Program	β_{11}	−0.839***	<0.001		
*Program * Calif*	β_{12}	−0.244	0.244		
*Program * Post*	β_{13}	−0.221*	0.098		
*Program * Calif * Post*	β_{14}	0.356	0.192		
ΔProgram	β_{11}			0.042***	<0.001
*ΔProgram * Calif*	β_{12}			−0.240	0.379
*ΔProgram * Post*	β_{13}			0.044	0.592
*ΔProgram * Calif * Post*	β_{14}			0.412	0.239
Industry Controls?		Yes		Yes	
Adj. R^2		0.435		0.008	
n		18,832		11,997	

***, **, * Indicate that coefficients are statistically different from 0 at the 1 percent, 5 percent, and 10 percent levels, respectively (two-tailed tests).

The sample is based on the observations for the period 2000–2010, excluding 2004, as defined in Table 1. Δ refers to change variables. Industry controls are employed through dummy variables defined in terms of one-character National Taxonomy of Exempt Entities (NTEE1) Codes. ∼+ (∼−) indicate coefficients that are positive (negative), but approximate to 0 when rounded to three decimal places.

All variables are defined in Table 3.

instance, stricter financial reporting requirements (Sections 12586(e)1 and 12586(e)2) and stricter contracting requirements with respect to fundraising activities (Section 12599(i)) likely increase the CEO's job responsibilities, even though these activities are not directly related to the nonprofit's charitable output. To the extent that the CEO needs to be compensated for the added administrative workload, it could explain the higher post-Act CEO compensation observed in Section III.

To test this conjecture, we need to distinguish between nonprofits that face high versus low regulation costs. We do so by using accounting fees as a key observable cost component that was directly impacted by the Act. Neely (2011) reports a significant increase in accounting fees for affected California nonprofits for the year that immediately follows the Act's implementation. Accordingly, we use the percentage increase in accounting fees from the year before to the year after the implementation of the Act to capture the Act's regulatory costs. Based on the median of the percentage in accounting fee change, we split the sample into two groups termed *High_Cost* and *Low_Cost,* where *High_Cost* (*Low_Cost*) represents nonprofits with higher (lower) than median changes in accounting fees. We then rerun Model (1) for each group separately. A larger positive coefficient on the interaction term *Calif* * *Post* (α_3) for the *High_Cost* group, when compared with that of the *Low_Cost* group, would provide evidence consistent with our regulatory costs conjecture.

These results are reported in Table 5 and are consistent with our assertion. The first (second) column of Table 5 reports results for the *High_Cost* (*Low_Cost*) group. The coefficient on *Calif* * *Post* (α_3) is positive and significant for *High_Cost* group ($\alpha_3^{High\_Cost} = 0.090$, $p = 0.017$), but is statistically insignificant for *Low_Cost* group ($\alpha_3^{Low\_Cost} = -0.012$, $p = 0.451$). Thus, it appears that the higher post-Act CEO pay is confined only to affected firms that likely faced higher regulatory costs. Moreover, the coefficient $\alpha_3^{High\_Cost}$ is significantly larger than the coefficient $\alpha_3^{Low\_Cost}$ (p-value = 0.081). These results suggest that burdensome cost of regulation may be a credible explanation for the increased post-Act CEO compensation detected in tests of H1.

Impact of the California Nonprofit Integrity Act of 2004 on Previously Underpaid Executives

While the Act mandates the boards to ensure that the CEO and CFO salaries are "just and reasonable," it does not postulate any guidelines to determine what qualifies as just and reasonable. It is possible that the Act might induce directors to simply seek the services of compensation consultants and/or introduce compensation benchmarking practices merely to justify existing compensation levels. A number of studies document this behavior in the corporate sector (for example, see Bebchuk and Fried 2003; Faulkender and Yang 2010; Wade, Porac, and Pollock 1997). As argued by Nagel (2007), these benchmarking exercises could highlight the instances where executives appear to be underpaid, and may pave the way for pay hikes within this subset of firms. In contrast, pay reductions when executives are overpaid are less likely, as attempts to do so are frequently met with stiff resistance by managers, and strategies such as selective choice of peer group benchmarks can be used to justify existing pay levels. Hence, the aggregate effect of benchmarking will be an increase in executive compensation. Consistent with this notion, Nagel (2007) finds that CEO pay for a given level of performance in the corporate sector has increased since the introduction of detailed pay surveys in the 1980s, and that the proportion of CEOs who are persistently underpaid has decreased. On the other hand, the proportion of CEOs who are overpaid has not changed.

Accordingly, we investigate whether the higher post-Act CEO compensation observed in tests of H1 is due to pay levels increasing for CEOs who are previously underpaid, while those of previously overpaid CEOs remain relatively unchanged. To do so, we follow the corporate sector literature and estimate the expected level of CEO compensation as a linear function of a number of explanatory variables (Core, Guay, and Larcker 2008) by regressing the log of the last pre-Act year's CEO compensation on *LnTA, Complex, Endow, Donations, Excess, Program, StateInc, StateGov,* and industry fixed effects. We run separate regressions for the California and control group nonprofits, and identify nonprofits with below (above) median residuals from this model as likely to be underpaying (overpaying) their CEOs in pre-Act periods.[18] We then run Model (1) on

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[18] Median and standard deviation of the residual for California (control group) are 0.039 and 0.479 (0.056 and 0.633), respectively. The means are zero by construction.

TABLE 5

Impact of the California Nonprofit Integrity Act of 2004 on Executive Compensation due to Costs of Regulation

$$
\begin{aligned}
LnComp_{it} = {} & \alpha_0 + \alpha_1 Calif_i + \alpha_2 Post_t + \alpha_3 Calif_i{*}Post_t + \alpha_4 LnTA_{it} + \alpha_5 Complex_{it} + \alpha_6 Endow_{it} \\
& + \alpha_7 Donations_{it} + \alpha_8 Excess_{it} + \alpha_9 StateInc_{it} + \alpha_{10} StateGov_{it} + \alpha_{11} Program_{it} \\
& + \alpha_j Industry_i + \varepsilon_{it}.
\end{aligned}
$$

Variable		***High_Cost***		***Low_Cost***	
		Coefficient	**p-value**	**Coefficient**	**p-value**
Intercept	α_0	2.879***	0.006	1.331	0.167
Calif	α_1	−0.031	0.579	0.030	0.401
Post	α_2	0.061	0.263	0.085*	0.065
*Calif * Post*	α_3	0.090**	0.017	−0.012	0.451
LnTA	α_4	0.037*	0.069	0.056**	0.016
Complex	α_5	0.021**	0.022	0.021**	0.944
Endow	α_6	~−0.000*	0.051	−0.049***	0.030
Donations	α_7	−0.111*	0.019	−0.121**	0.027
Excess	α_8	0.016**	0.023	0.009	0.294
StateInc	α_9	0.324***	0.002	0.500***	<0.001
StateGov	α_{10}	−0.006	0.249	0.014***	0.005
Program	α_{11}	−1.126	<0.001	−0.764***	<0.001
Industry Controls?		Yes		Yes	
Adj. R^2		0.475		0.431	
n		6,223		6,999	

p-value of test on: $\hat{\alpha}_3^{High\_Cost} > \hat{\alpha}_3^{Low\_Cost} = 0.081$

***, **, * Indicate that coefficients are statistically different from 0 at the 1 percent, 5 percent, and 10 percent levels, respectively (two-tailed tests).

The sample is based on the observations for the period 2000–2010, excluding 2004, as defined in Table 1. Observations are separated into *High_Cost* and *Low_Cost* based on the percentage increase in accounting fees from the year before to the year after the implementation of the Act. Industry controls are employed through dummy variables defined in terms of one-character National Taxonomy of Exempt Entities (NTEE1) Codes. ~+ (~−) indicate coefficients that are positive (negative), but approximate to 0 when rounded to three decimal places.

All variables are defined in Table 3.

the *Underpaid* and *Overpaid* groups separately. If our assertion that the Act has resulted in larger pay increases for CEOs who were previously underpaid is correct, then the coefficient on the interaction term *Calif * Post* (α_3) should be greater for the *Underpaid* group in comparison to the *Overpaid* group.

The results for these tests are reported in Table 6. The first column of Table 6 reports results for the *Underpaid* group, while the second column reports those for the *Overpaid* group. We observe that α_3, the coefficient of interest on the interaction term *Calif * Post,* is positive, but not significant, for both the *Underpaid* and *Overpaid* groups ($\alpha_3^{Underpaid} = 0.039$, $p = 0.239$; $\alpha_3^{Overpaid} = 0.026$, $p = 0.243$). Moreover, we fail to find that the coefficient $\alpha_3^{Underpaid}$ is significantly larger than the coefficient $\alpha_3^{Overpaid}$ ($p = 0.781$). Thus, we do not find any evidence to indicate that the Act has had a disproportionate impact on affected nonprofits whose CEOs were likely to have been underpaid during pre-Act periods. In other words, the previously observed higher post-Act CEO salaries seem

TABLE 6

Impact of the California Nonprofit Integrity Act of 2004 on Previously Underpaid Executives

$$\begin{aligned} LnComp_{it} = {} & \alpha_0 + \alpha_1 Calif_i + \alpha_2 Post_t + \alpha_3 Calif_i{*}Post_t + \alpha_4 LnTA_{it} + \alpha_5 Complex_{it} + \alpha_6 Endow_{it} \\ & + \alpha_7 Donations_{it} + \alpha_8 Excess_{it} + \alpha_9 StateInc_{it} + \alpha_{10} StateGov_{it} + \alpha_{11} Program_{it} \\ & + \alpha_j Industry_i + \varepsilon_{it}. \end{aligned}$$

		Underpaid		*Overpaid*	
Variable		**Coefficient**	**p-value**	**Coefficient**	**p-value**
Intercept	α_0	1.112	0.258	1.521***	0.008
Calif	α_1	0.038	0.263	0.005	0.840
Post	α_2	0.205***	<0.001	–0.015	0.710
Calif * *Post*	α_3	0.039	0.239	0.026	0.243
LnTA	α_4	–0.007	0.758	0.080***	<0.001
Complex	α_5	0.050	<0.001	–0.016	<0.001
Endow	α_6	~–0.000	0.761	~0.000	0.943
Donations	α_7	–0.049	0.399	–0.088***	0.007
Excess	α_8	–0.004	0.576	0.008	0.110
StateInc	α_9	0.535***	<0.001	0.477***	<0.001
StateGov	α_{10}	0.007	0.191	0.009***	0.001
Program	α_{11}	–1.007***	<0.001	–0.999***	<0.001
Industry Controls?		Yes		Yes	
Adj. R^2		0.400		0.660	
n		6,985		8,753	

p-value of test on: $\hat{\alpha}_3^{Underpaid} > \hat{\alpha}_3^{Overpaid} = 0.781$

*** Indicates that coefficients are statistically different from 0 at the 1 percent level (two-tailed tests).

The sample is based on the observations for the period 2000–2010, excluding 2004, as defined in Table 1. Observations are separated into *Underpaid* and *Overpaid* groups based on whether the residuals from the model $LnComp_i = \gamma_0 + \gamma_1 LnTA_i + \gamma_2 Complex_i + \gamma_3 Endow_i + \gamma_4 Donations_i + \gamma_5 StateInc_i + \gamma_6 StateGov_i + \gamma_7 Program_i + \gamma j Industry_i + \varepsilon_i$, run separately for California and control group nonprofits for the year immediately preceding the implementation of the California Nonprofit Integrity Act, are below or above the cross-sectional medians. Industry controls are employed through dummy variables defined in terms of one-character National Taxonomy of Exempt Entities (NTEE1) Codes. ~+ (~–) indicate coefficients that are positive (negative), but approximate to 0 when rounded to three decimal places. All variables are defined in Table 3.

to be an across-the-board phenomenon for both previously underpaid and overpaid California executives.[19]

It is also interesting to note in Table 6 that the coefficient on *Post* (α_2) is positive and significant for the *Underpaid* group ($\alpha_2^{Underpaid} = 0.205$, $p < 0.001$), but statistically insignificant for the *Overpaid* group ($\alpha_2^{Overpaid} = -0.015$, $p = 0.710$). This finding lends support to Nagel's (2007) argument that salaries of underpaid CEOs are adjusted upward without an accompanying downward adjustment to overpaid CEOs' salaries. It appears that the Act has not changed this general phenomenon.

Taken together, the evidence reported in Section IV indicates added regulatory costs to be the most credible explanation for higher post-Act CEO pay in affected nonprofits.

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[19] Note that the coefficient on *Calif* * *Post* is statistically insignificant for both groups. This is likely due to lack of power. As reported in Table 3, the coefficient is positive and significant when the two subsamples are combined.

V. ADDITIONAL TESTS

This section discusses the results of a number of additional tests we have conducted with the objective of gaining further insights and ruling out alternative explanations.

Impact of the California Nonprofit Integrity Act of 2004 on Large versus Small Nonprofits

The Act in its entirety is applicable to nonprofits with gross revenues exceeding \$2 million. While these are relatively large organizations in the broader context of the not-for-profit sector, there is substantial cross-sectional variation among affected nonprofits in terms of size.[20]

The overall impact of the Act and its executive compensation implications are likely to be more pronounced for relatively smaller nonprofits. The operating practices of larger nonprofits are more likely to have been aligned with the key provisions of the Act since the pre-Act period. For instance, larger entities are more likely to have submitted audited financial statements prior to the Act, as audited financial statements are required for securing large government grants (of over \$500,000) and are usually demanded by other large donors. Ostrower and Bobowick (2006) and Vermeer, Raghunandan, and Forgione (2006) report that larger nonprofits are more likely to have audit committees. Moreover, practices such as peer group benchmarking are more likely to have already been in place for larger nonprofits. Accordingly, Gilkeson (2007) argues that administrative and reporting burdens of regulations of this nature are especially costly for smaller nonprofits. Therefore, we expect the effect of the Act in terms of higher CEO pay to be stronger for affected nonprofits that are relatively smaller.

We test this conjecture by separating our sample into two groups of *Small* and *Large* entities based on median revenue, and rerunning Model (1) for each group separately. We predict the coefficient on the interaction term *Calif* $*$ *Post* (α_3) to be greater for the *Small* group when compared with that for the *Large* group. These results are reported in Table 7, with the first (second) column reporting results for the *Small* (*Large*) nonprofits. The results provide some, albeit weak, support for our conjecture. Consistent with our argument, we find the coefficient on the interaction term *Calif* $*$ *Post* (α_3) to be statistically significant for the *Small* group, but not the *Large* group ($\alpha_3^{Small} = 0.064$, $p = 0.069$; $\alpha_3^{Large} = 0.053$, $p = 0.143$). However, while the estimated coefficient α_3^{Small} is larger than the estimated coefficient α_3^{Large}, this difference is not statistically significant (p-value = 0.832).

Impact of the California Nonprofit Integrity Act of 2004 on Program Ratio

While the results reported in Section IV appear to rule out higher post-Act pay performance sensitivity as an explanation for higher pay observed in results of H1, it could be argued that the governance improvements introduced by the Act have improved the performance levels of affected nonprofits while keeping the pay performance sensitivities relatively unchanged, and that the higher executive compensation is likely a reward for this improved performance. We test this alternative explanation by regressing the program ratio on dummy variables *Calif, Post,* and the interaction term *Calif* $*$ *Post.* We use *LnTA, Complex, Endow, Donations, Excess, StateInc,* and *StateGov* as control variables, along with industry fixed effects. If the Act led to a performance improvement in affected nonprofits, then the coefficient on the interaction term *Calif* $*$ *Post* is expected to be positive and significant. However, our results (untabulated) indicate that this coefficient is not statistically significant (coefficient magnitude = 0.001, p-value = 0.924). Hence, higher post-Act performance levels do not appear to be a credible explanation for our H1 results.

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[20] For instance, untabulated descriptive statistics indicate that the total asset size of the 10th percentile of affected nonprofits is US\$5.2 million, while that of the 90th percentile is as much as US\$170 million. Note that these numbers have been obtained after outliers have been removed from the sample.

TABLE 7

Impact of the California Nonprofit Integrity Act of 2004 on Large versus Small Nonprofits

$$LnComp_{it} = \alpha_0 + \alpha_1 Calif_i + \alpha_2 Post_t + \alpha_3 Calif_i{*}Post_t + \alpha_4 LnTA_{it} + \alpha_5 Complex_{it} + \alpha_6 Endow_{it} + \alpha_7 Donations_{it} + \alpha_8 Excess_{it} + \alpha_9 StateInc_{it} + \alpha_{10} StateGov_{it} + \alpha_{11} Program_{it} + \alpha_j Industry_i + \varepsilon_{it}.$$

Variable		*Small*		*Large*	
		Coefficient	**p-value**	**Coefficient**	**p-value**
Intercept	α_0	1.065	0.111	2.800***	0.002
Calif	α_1	−0.032	0.300	0.0008	0.865
Post	α_2	0.018	0.681	0.139**	0.029
*Calif * Post*	α_3	0.064*	0.069	0.053	0.143
LnTA	α_4	0.037***	<0.001	0.035***	<0.001
Complex	α_5	0.020***	0.002	0.029***	0.002
Endow	α_6	~0.000	0.504	~0.000	0.341
Donations	α_7	−0.094***	0.003	−0.110	0.103
Excess	α_8	0.016***	0.003	0.005	0.502
StateInc	α_9	0.518***	<0.001	0.427***	<0.001
StateGov	α_{10}	0.001	0.819	0.011**	0.023
Program	α_{11}	−0.945***	<0.001	−1.030***	<0.001
Industry Controls?		Yes		Yes	
Adj. R^2		0.329		0.367	
n		9,418		9,414	

p-value of test on: $\hat{\alpha}_3^{Small} > \hat{\alpha}_3^{Large} = 0.832$

***, **, * Indicate that coefficients are statistically different from 0 at the 1 percent, 5 percent, and 10 percent levels, respectively (two-tailed tests).

The sample is based on the observations for the period 2000–2010, excluding 2004, as defined in Table 1. Observations are separated into *Small* and *Large* groups based on revenue. Industry controls are employed through dummy variables defined in terms of one-character National Taxonomy of Exempt Entities (NTEE1) Codes. ~+ (~−) indicate coefficients that are positive (negative), but approximate to 0 when rounded to three decimal places.

All variables are defined in Table 3.

Improved Reporting of CEO Compensation as a Potential Confound

Prior research suggests that nonprofit managers use their reporting discretion to generate more desirable financial outcomes (e.g., see Jones and Roberts 2006; Krishnan et al. 2006). As some key provisions of the Act, such as mandating of audited financial statements and establishment of audit committees, were directly aimed at improving the integrity of nonprofits' financial reports, it could be argued that the observation of higher post-Act CEO salaries for affected entities is merely due to these being more accurately reported as a result of the Act. To rule out this possibility, we test whether the compensation reporting quality has improved post-Act by following Neely's (2011) approach. Specifically, we compare the incidence of discrepancies between the compensation of officers as reported on line 25 of Form 990, and the detailed disclosures on Schedule V of Form 990 during pre- and post-Act periods. As in Neely (2011), we do not find a significant reduction in these discrepancies following the enactment of the Act. This suggests that our main finding is unlikely to be an artifact of improved financial reporting quality in post-Act periods.

Higher CEO Turnover as a Potential Confound

To the extent that new CEOs tend to get paid more than their predecessors, it is possible that the observed phenomenon of higher post-Act CEO salaries in affected nonprofits is due to higher turnover rates than in the control group.[21] In order to address this concern, we have manually collected the CEO names from the NCCS digitized database and Form 990s for our entire California sample and isolated the cases where the CEO has changed. If the concern is that new CEOs get paid more, then removing these cases from the California sample should bias against our findings. However, our results remain unchanged when we do so, suggesting that they are not driven by higher CEO turnover rates in California.

VI. CONCLUSION

In this paper, we assess whether and how California's Nonprofit Integrity Act of 2004 has influenced the executive compensation costs of affected charitable organizations. Given the general concerns raised by regulators over the potentially excessive executive compensation in nonprofits and the specific requirement of the Act aimed at ensuring that executive compensations are "just and reasonable," it is reasonable to argue that regulators intended the Act to have a moderating effect on executive compensation. However, our findings indicate that this has not been the case. When compared with a control group of similar nonprofits from other states in the U.S., we find that for affected firms, the level of CEO pay has increased. Moreover, we do not find the Act to have resulted in an increase in CEO pay performance sensitivity. Consistent with the notion that the regulation has increased the workload of the CEO in areas not directly related to charitable activities of the entity, we find the CEO compensation impact of the Act to be greater for nonprofits that likely faced higher regulatory costs. The reported increase in CEO compensation seems to be across the board and not confined to entities where CEOs were likely underpaid in pre-Act periods. In additional tests, we also find some evidence that higher post-Act CEO pay is primarily attributed to smaller affected nonprofits, suggesting that the impact of the Act was mainly felt by relatively smaller entities, as larger nonprofits were likely to have been already following many of the Act's recommendations.

We believe our findings have broad implications for informing the policy debate on the relative merits of regulating governance in the not-for-profit sector, as a number of other states have either adopted similar regulations or are contemplating doing so (Mead 2008). Moreover, our findings are also consistent with some concerns raised in the corporate sector that attempts to regulate executive compensation can lead to unintended consequences (Cohen, Dey, and Lys 2008; Romano 2005).

However, we would like to add the following caveats to our findings. First, our findings should not be interpreted as a comprehensive repudiation of the efficacy of the Act. Our study is focused on a specific facet of the Act that we believe will be of interest to researchers, as well as practitioners and policy makers. However, in our opinion, the extant body of literature is not sufficient to make broad claims on the overall efficacy of this piece of legislation.

Second, while our findings can be interpreted as having broader implications for the debate on governance regulation in both the not-for-profit and corporate sectors, we caution against over-generalizations, as specific institutional and environmental settings can either moderate or intensify the effects uncovered in this paper. We leave further investigation of these aspects to future researchers.

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[21] We thank an anonymous referee for pointing this out.

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The Robustness of Honesty Effects on Budget Proposals when the Superior has Rejection Authority

Jeremy D. Douthit
The University of Arizona

Douglas E. Stevens
Georgia State University

ABSTRACT: Rankin, Schwartz, and Young (2008) find experimental evidence that manipulating whether the budget request of the subordinate requires a factual assertion has no effect on budgetary slack when the superior can reject the budget. This calls into question the role of honesty in participative budgeting settings. Using Rankin et al.'s (2008) manipulation to capture honesty effects, we examine the robustness of honesty effects on budget proposals when the superior has rejection authority in two experiments. In Experiment 1, we document that honesty has a strong effect on budgetary slack when the salience of distributional fairness is reduced by withholding the relative pay of the superior from the subordinate. In Experiment 2, we document that honesty continues to have a strong effect on budgetary slack when the salience of reciprocity is increased by giving the superior the ability to set the subordinate's salary. Thus, our evidence suggests that honesty effects on budget proposals are generally robust to giving the superior rejection authority. Our study helps explain prior experimental results and clarifies the role of honesty in participative budgeting settings.

Keywords: *budgetary slack; honesty; distributional fairness; reciprocity.*

Data Availability: *Experimental data are available from the authors upon request.*

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I. INTRODUCTION

Participative budgeting represents one of the most widely researched topics of experimental research in managerial accounting (Brown, Evans, and Moser 2009). This body of research is important because participative budgeting is commonly used to aid in the efficient allocation of resources in decentralized firms (J. Shields and M. Shields 1998; Libby and Lindsay 2007). To the extent that subordinates truthfully communicate their private information in the budget, participative budgeting yields useful information for central management to use in production, marketing, and capital budgeting decisions (Evans, Hannan, Krishnan, and Moser 2001). Traditional agency theory, however, assumes that subordinates suffer no disutility for misrepresenting their private information and will build slack into their budget to maximize their wealth, absent a contractual incentive to do otherwise (Baiman 1990). From this perspective, budgetary slack is a type of opportunistic behavior that reduces the value of participative budgeting to the firm (Baiman and Evans 1983; Merchant 1985; Kren and Liao 1988).[1]

Contrary to the assumptions of traditional agency theory, prior experimental studies of participative budgeting find that subordinates exhibit honesty concerns in their budget proposals (Young 1985; Chow, Cooper, and Waller 1988; Evans et al. 2001; Stevens 2002; Hannan, Rankin, and Towry 2006; Zhang 2008).[2] These studies also find that such honesty concerns can lead to increased efficiency for the firm in participative budgeting settings (Evans et al. 2001).[3] This suggests that honesty may support the underlying value of participative budgeting to the firm. However, surveys of managers and accountants reflect continuing concerns that subordinates may strategically manipulate the budgeting process to the detriment of the firm (Neely, Sutcliff, and Heyns 2001; Libby and Lindsay 2007). Jensen (2001, 96) characterizes participative budgeting as a game that "encourages managers to lie and cheat." This view, however, does not explain the continued popularity of participative budgeting as an organizational control (Shields and Shields 1998; Libby and Lindsay 2010).

Recent experimental evidence in Rankin, Schwartz, and Young (2008; hereafter, RSY) calls into question the role of honesty in participative budgeting settings. RSY note that previous experimental studies finding evidence for honesty use budgetary slack as a direct measure of honesty and grant the subordinate unilateral budget authority. They argue that budgetary slack is influenced by other non-pecuniary preferences besides honesty, such as preferences for the distribution of wealth, and that granting unilateral budget authority may cause the subordinate to frame the budgeting setting as an ethical dilemma. RSY contribute to the literature by attempting to isolate the incremental effect of honesty on budgetary slack and by examining budgeting settings where the superior can reject the subordinate's budget. RSY's experimental design isolates the incremental effect of honesty by manipulating the mode of communication of the budget proposal, such that subordinates either provide a factual assertion of actual project cost or propose a division of the project earnings without a factual assertion. RSY find less budgetary slack when the budget communication requires a factual assertion in the setting where the subordinate has unilateral authority, but not when the superior has rejection authority. Exit questionnaire data suggest that their result is due to subordinates framing the setting where the superior has rejection authority as a strategic negotiation rather than an ethical dilemma.

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[1] The literature frequently distinguishes between budgetary slack and organizational slack. Budgetary slack occurs when expense budgets are overstated or when revenue or production budgets are understated due to managers' incentives to outperform the budget, thereby leading to inefficiencies for the firm (Kren and Liao 1988). Organizational slack can be viewed positively as a response to uncertainty that provides a stabilizing influence in down times due to the availability of margin (Cyert and March 1963; Merchant 1985).

[2] Prior experimental studies use the honesty construct to explain the tendency of a person to make true statements despite incentives to make false statements (Evans et al. 2001; Rankin et al. 2008). Consistent with economic models that incorporate honesty in a capital budgeting setting, we define honesty as a preference that causes disutility from making false statements (Mittendorf 2006).

[3] It may not be honesty *per se* that motivates honest behavior, but simply the desire to be perceived as honest (Hannan et al. 2006). As with prior studies, we do not distinguish between these two motivations in our study.

Because participative budgeting settings in practice typically include superior rejection authority, it is important to examine the robustness of honesty effects on budget proposals in this enhanced setting. RSY (2008, 1085) conclude that their results are consistent with extrinsic control in the form of superior rejection authority "crowding out" intrinsic motivation to display honesty in the budget. This is consistent with recent social norm theory suggesting that situational cues play an important role in activating social norms and determining which norm will dominate in the presence of conflicting norms (Bicchieri 2006). We identify three social norms that may be activated and affect budgetary slack in a participative budgeting setting where the superior has rejection authority: honesty, distributional fairness, and reciprocity. Consistent with economic models incorporating honesty in a capital budgeting setting, we define honesty as a preference that causes disutility from making false statements (Mittendorf 2006). Consistent with economic models incorporating social preferences in game theory, we define distributional fairness as a preference over the relative distribution of outcomes that causes disutility from inappropriate allocations of an available surplus (Fehr and Schmidt 1999; Falk, Fehr, and Fischbacher 2003) and reciprocity as a preference that causes positive utility from repaying kind actions with kindness and unkind actions with unkindness (Rabin 1993; Cox and Deck 2005). To examine the robustness of honesty effects on budget proposals when the superior has rejection authority, we manipulate the salience of distributional fairness and reciprocity norms in two experiments.

In Experiment 1, we examine the effect of distributional fairness on honesty effects by manipulating the ability of the subordinate to discern the level of pay the superior receives relative to the subordinate's level of pay. In typical budgeting settings, the relative distribution of outcomes from a given project and budget decision is less transparent than in laboratory settings. This transparency is an important factor to consider, since the participative budgeting setting is economically equivalent to an ultimatum game when the superior has rejection authority (RSY 2008), and experimental studies of ultimatum games document that distributional fairness becomes salient when relative pay information is transparent (Croson 1996; Guth, Huck, and Muller 2001; Charness and Gneezy 2008). When relative outcomes are not transparent, however, the salience of distributional fairness is reduced (Dana, Weber, and Kuang 2007). Recent social norm theory (Bicchieri 2006, 131) suggests that situational cues determine which norm is most salient and will dominate behavior in the presence of conflicting norms. Thus, while RSY manipulated the salience of honesty by manipulating the form of the budget communication, the transparency of relative pay in their study may have made distributional fairness more salient than honesty. Experiment 1 addresses this research question by testing the robustness of honesty effects under superior rejection authority where the salience of distributional fairness is manipulated.

In Experiment 2, we examine the effect of reciprocity on honesty effects by manipulating the ability of the superior to set the subordinate's pay while reducing the salience of distributional fairness, as in Experiment 1. While superiors typically play a role in setting the subordinate's pay in practice, researchers have only recently examined this important aspect of participative budgeting. Using a group setting where the superior sets the salary of two subordinates, Zhang (2008) finds evidence of reciprocity in that subordinates build less slack into their budget when the superior offers higher pay. However, as with most studies in the literature, Zhang (2008) uses budgetary slack as a direct measure of honesty and does not give superiors rejection authority.[4] Thus, while her study documents that reciprocity affects budgetary slack, it remains to be determined whether honesty affects budgetary slack under superior rejection authority in a budgeting setting where reciprocity is salient. Experiment 2 addresses this research question by testing the robustness of honesty effects under superior rejection authority where the salience of reciprocity is manipulated.

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[4] Other ways in which our study is distinguishable from Zhang (2008) are that she investigates a multi-agent setting and focuses on the role of wage levels and inter-agent communication on budgetary slack and whistleblowing.

In both experiments, we utilize a slightly modified version of RSY's experimental setting where the superior has rejection authority.[5] Following the strategic budgeting setting in Schatzberg and Stevens (2008), we pay the subordinate's fixed salary from the superior's endowment rather than from the experimenter, as in RSY. This design choice is likely to capture an important aspect of budgeting settings where the resources of the firm are limited and the superior is thereby motivated to be efficient with those resources. As in RSY, we capture the incremental effect of honesty on budgetary slack as the difference between a budget proposal that requires a factual assertion about the cost of the project and a budget proposal that simply offers the superior a portion of the profit from the project without a factual assertion.

In Experiment 1, we document an interaction between distributional fairness and honesty whereby the incremental effect of honesty on budgetary slack is stronger when the salience of distributional fairness is reduced by keeping the level of pay the superior receives relative to the subordinate private and known only by the superior. When the superior's relative pay is private, honesty has a strong incremental effect on budgetary slack. When the superior's relative pay is common knowledge, as in RSY, honesty still has a marginally significant effect on budgetary slack. Thus, distributional fairness significantly weakens, but does not completely dominate, honesty in our experimental setting. In Experiment 2, we document that honesty effects on budgetary slack are robust to the salience of reciprocity. When the superior has the authority to set the subordinate's salary, subordinates create less budgetary slack when the superior offers a higher salary, as in Zhang (2008). Nevertheless, honesty continues to have a strong incremental effect on budgetary slack in this experimental setting.

Giving the superior rejection authority and the authority to set the subordinate's salary in Experiment 2 allows us to examine how honesty and reciprocity norms affect the rejection and salary behavior of the superior. We find that, after controlling for the reported budget level, the probability of rejection is higher when the budget communication requires a factual assertion, consistent with superiors being motivated to enforce honesty even at a cost to themselves (Hannan, Rankin, and Towry 2010). Further, the probability of rejection is higher when the superior sets a higher salary, consistent with superiors being motivated to enforce reciprocity even at a cost to themselves. Finally, we find that reciprocity alone is insufficient to control the subordinate's opportunism. While subordinates claim less slack as their pay increases, the decrease in slack does not completely offset the cost of the increased pay for the superior. Thus, even in the likely setting where the superior influences the subordinate's pay and reciprocity is present, honesty plays an important role in maintaining the efficiency of participative budgeting.

In summary, our evidence suggests that honesty effects on budget proposals are generally robust to giving the superior rejection authority. Our study helps explain prior experimental results in participative budgeting. For example, results from Experiment 1 suggest that RSY's experimental result that honesty does not have an incremental effect on budgetary slack under superior rejection authority may be partially attributable to the salience of distributional fairness in their study. While we document that distributional fairness can diminish honesty effects in experimental budgeting settings, we do not expect distributional fairness to be especially salient in budgeting settings in practice because the superior's relative pay is typically not transparent to the subordinate. It is more likely that reciprocity will be salient because the superior typically plays a role in setting the subordinate's pay. Results from Experiment 2 suggest that reciprocity does not diminish honesty effects and superiors can achieve higher profits by relying on such honesty. Thus, our results support prior experimental research documenting the importance of honesty in maintaining the efficiency of participative budgeting (Brown et al. 2009).

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[5] We contacted one of the RSY (2008) authors and requested their original set of instructions. Their gracious willingness to provide us their instructions allowed us to closely replicate their experimental setting aside from the changes noted.

Our study also provides useful evidence for accounting theory and practice. Theorists in management accounting have traditionally used agency theory and the "nexus of contracts" view of the firm made popular in finance (Jensen and Meckling 1976). While this view of the firm has generated valuable insights and useful streams of research in accounting (Baiman 1990), it has not always been able to describe management practices and controls found in practice (Stevens and Thevaranjan 2010). Despite the potential for participative budgeting to enhance information sharing, planning, and control, some agency theorists have minimized the value of participative budgeting due to the powerful incentive to misreport private information for personal gain (Jensen 2001). Our results help explain why firms have historically used participative budgeting for both performance evaluation and control purposes despite the incentive for misreporting (Shields and Shields 1998; Libby and Lindsay 2010). Our study also suggests that the value of participative budgeting to the firm could be enhanced by incorporating contextual cues and behavioral expectations that motivate honest reporting of private information in the budget.

Next, in Section II, we present the theoretical foundation of our study. In Section III, we develop our hypotheses for Experiment 1, describe our experimental design, and present our results. In Section IV, we develop our hypotheses for Experiment 2, describe our experimental design, and present our results. In Section V, we conclude.

II. THEORETICAL FOUNDATION

For the theoretical foundation of our study, we utilize the agency model of participative budgeting in Antle and Eppen (1985) and incorporate insights from a model of social norm activation in Bicchieri (2006). Antle and Eppen's (1985) agency model is commonly used by experimental researchers to examine the effect of honesty in participative budgeting. In their model, there is an investment project that is being considered by a firm. Both the subordinate and the superior know the revenues that will result from the project if it is undertaken and the probability distribution of the project's cost. However, only the subordinate learns the actual cost of the project. Upon learning the actual cost of the project, the subordinate submits a cost budget proposal to the superior. The superior then decides whether to fund the project. If the project is funded, then the superior receives the profit above the cost budget and the subordinate receives the slack between the cost budget and actual cost. Absent some financial incentive or hurdle contract, the agency prediction is for the subordinate to submit the highest cost budget possible, regardless of the actual cost, and for the superior to accept any project that has a positive profit. These strong predictions are due to the traditional agency theory assumption that the subordinate and superior are motivated solely by their own wealth and leisure.

There are several features of the theoretical setting in Antle and Eppen (1985) that make it ideal for testing the effect of honesty on the budget proposals of subordinates. First, the model provides a strong agency prediction that can be used to test alternative theories of behavior (Brown et al. 2009). Second, the subordinate knows the actual cost with certainty when he proposes his budget, which controls for risk preferences (RSY 2008). Third, social pressure, reputation, and other multi-period effects that have been shown to affect budgetary slack (Young 1985; Stevens 2002) are controlled because the model is a single-period model and the superior never learns the true cost of the project.

We supplement Antle and Eppen's (1985) agency model with insights from Bicchieri's (2006) model of social norm activation. Researchers in accounting have begun to reference Bicchieri's

(2006) model to predict how and when environmental cues activate social norms that affect the behavior of the principal and the agent (Douthit, Kearney, and Stevens 2012; Davidson and Stevens 2013).[6] A social norm is a behavioral rule that may become salient and affect behavior in a given social setting. Bicchieri (2006) argues that people have conditional preferences for conforming to social norms. In her model, a social norm is activated and followed when one becomes aware that a given norm is relevant to the current setting, and a combination of empirical and normative expectations gives one a sufficient reason to follow the norm. Empirical expectations are based on the belief that a sufficiently large subset of people conforms to the norm in similar settings. Normative expectations are based on the belief that a sufficiently large subset of people expects conformance to the norm in similar settings and may be willing to sanction behavior inconsistent with the norm. Therefore, a given social norm is activated when situational cues make the norm salient and the norm shapes empirical and normative expectations of behavior.[7]

Bicchieri's (2006, 131) model predicts that in the common setting where multiple social norms are present, situational cues play an important role in determining which norm will be most salient and influence behavior. One of the most prominent social norms in society is honesty (Murphy 1993). Bicchieri's (2006) model suggests that the mode of budget communication manipulation used by RSY makes the honesty norm salient and, therefore, is a valid measure of the effect of honesty on budgetary slack. Besides honesty, we identify two other social norms that may be activated in a participative budgeting setting where the superior has rejection authority: distributional fairness and reciprocity. Below, we incorporate insights from Bicchieri's (2006) model of social norm activation to Antle and Eppen's (1985) agency model to predict how and when situational cues in the budgeting setting make distributional fairness and reciprocity norms salient and their differential effect on honesty in a participative budgeting setting.

III. EXPERIMENT 1: HONESTY AND DISTRIBUTIONAL FAIRNESS

Hypothesis Development for Experiment 1

In Experiment 1, we examine the robustness of honesty effects on budget proposals when the superior has rejection authority by manipulating the salience of distributional fairness. When the superior has rejection authority, the budgeting setting resembles an ultimatum game where a proposer offers a split of a surplus to a responder and the responder can either accept or reject the split (RSY 2008, 1088). If the responder rejects the offer, then neither party receives any pay. Guth, Schmittberger, and Schwarze (1982) developed the ultimatum game to test predictions of game theory and examine preferences for distributional fairness (Camerer 2003; Smith 2010). Experimental tests of the ultimatum game find that when relative payoff information is transparent, distributional fairness affects the behavior of both the proposer and responder. In contrast to the traditional game theory prediction that the proposer will offer the minimal share of the surplus and it will be accepted by the responder, proposers commonly offer 30–50 percent of the surplus and responders commonly reject offers below 20 percent (Croson 1996; Hoffman, McCabe, and Smith 2000; Guth et al. 2001; Charness and Gneezy 2008; Stanca 2010).[8]

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[6] For a formal presentation of Bicchieri's (2006) model and a detailed discussion of its usefulness to experimental research in accounting, see Davidson and Stevens (2013).

[7] While empirical expectations aid in social norm activation, it is possible for normative expectations alone to activate a social norm, provided an individual recognizes this expectation as legitimate in and of itself (Bicchieri 2008). Also, normative expectations may induce conformance to the social norm without the threat of sanctions (Bicchieri 2006).

[8] The game theoretic prediction is based on the traditional agency assumption that participants are motivated solely by their own wealth and leisure. For reviews of the literature, see Guth (1995) and Cooper and Kagel (2015).

In the traditional ultimatum game, the surplus that the proposer splits with the responder is an endowment or monetary gift from the experimenter that is common knowledge to both parties. The default expectation generated by distributional fairness in this setting is an equal-split norm (Bicchieri 2006). In asking herself what the responder would accept, the proposer is forced to look at the cues in the decision setting and conclude that there is an expected opportunity cost of not following the equal-split norm. However, changes in the decision setting have been found to alter distributional fairness expectations in both the proposer and the responder. Responders are willing to accept lower offers from the proposer when such offers are generated by a random device or the choice of the proposer is otherwise constrained (Blount 1995). Research also suggests that distributional fairness is defined in a self-serving manner, as proposers offer lower shares of the surplus when they possess advantageous private information that makes such offers appear fair to the responder (Kagel, Kim, and Moser 1996). Finally, research shows that proposers are willing to restrict their information to avoid having to consider the distributional fairness of their offer (Dana et al. 2007).

In summary, research suggests that the transparency of relative payoffs is a powerful situational cue that makes distributional fairness salient in settings that resemble an ultimatum game. In participative budgeting settings in practice, subordinates are typically unaware of the pay of their superiors or how a given project and budget influences this pay. Thus, distributional fairness is unlikely to be as salient in budgeting settings in practice as in experimental budgeting settings where relative pay is transparent. Experimental studies in economics (Carpenter 2002; Dana et al. 2007) and accounting (Kachelmeier and Towry 2002; Davidson and Stevens 2013) reduce the salience of distributional fairness by keeping relative pay information hidden from participants. Therefore, we manipulate the salience of distributional fairness by disclosing or withholding information about the relative pay of the superior. In one condition, the superior's initial endowment is common knowledge to both parties, and in the other condition, the superior's initial endowment is known only by the superior.

We argue that honesty and distributional fairness represent conflicting social norms in a participative budgeting setting. Prior experimental evidence suggests that individuals use dishonest actions to both help and harm others in order to create more equitable outcomes (Gino and Pierce 2010). This offsetting relation between honesty and distributional fairness is likely to be present in an experimental budgeting setting because the subordinate affects the distribution of the project surplus through the honesty of his budget report (RSY 2008, 1084). To the extent that honest reporting leads to inequitable outcomes, equitable outcomes can only be achieved through dishonest reporting. That is, an honest report gives the entirety of the production surplus to the superior, and the only avenue available for the subordinate to receive an equitable share of the production surplus is to report dishonestly. This is often the case with experimental budgeting settings, such as RSY.

Bicchieri's (2006, 131) model predicts that where conflicting social norms are present, expectations and behavior will be affected by the most salient of the social norms. In experimental budgeting settings, this suggests that participants must assess a probability distribution over the range of possible social norms that will be activated. As situational cues make distributional fairness more salient, it increases the probability that this social norm will be activated and dominate honesty. Consistent with ultimatum game results, we expect the disclosure of relative payoff information to be a powerful situational cue that makes distributional fairness salient in an experimental budgeting setting. Thus, we manipulate the salience of distributional fairness by manipulating the transparency of relative pay information to the subordinate, and we expect honesty effects on budgetary slack to be diminished when relative pay information is transparent. Based on Bicchieri's (2006) model and prior experimental evidence, in H1, we predict that honesty will have a greater effect on budgetary slack when the relative pay of the superior is withheld from the subordinate:

H1: Honesty will have a greater effect on budgetary slack when the relative payoff of the superior is withheld from the subordinate.

Experimental Design for Experiment 1

To test our first hypothesis, we use a controlled laboratory experiment that contains key aspects of Antle and Eppen's (1985) model. Upon arriving at the laboratory, participants were randomly assigned to the role of either a superior or a subordinate.[9] They retained their role for the entire experiment. The following steps took place in each period of the experiment. The experimental task involved an investment project that yielded revenue of 200 experimental dollars when funded. The cost of the investment was uniformly distributed between 0–200 experimental dollars in increments of 1. The revenue and cost distribution of the project were common knowledge. The superior received an initial endowment of 100 experimental dollars. The subordinate received a salary of 100 experimental dollars from the superior's initial endowment[10] and observed the actual cost of the project. Upon learning the actual cost, the subordinate proposed a budget to the superior. The superior then decided whether to fund the project. If the project was funded, then the subordinate received the reported cost less the actual cost (budgetary slack) and the superior received the project revenue (200 experimental dollars) less the reported cost. If the project was not funded, then neither party received any payoff from the project. However, the subordinate received a salary from the superior's endowment of funds regardless of whether the project was funded. Participants were randomly and anonymously rematched with each other after each period. There were 20 periods.

Experiment 1 uses a 2 × 2 factorial design where the subordinate's knowledge of the superior's initial endowment level is manipulated at two levels (Public Superior Pay versus Private Superior Pay) and the mode of budget communication is manipulated at two levels (No Factual Assertion versus Factual Assertion). This experimental design is presented in Figure 1. We manipulate the subordinate's knowledge of the superior's initial endowment to manipulate the salience of distributional fairness. The superior paid the subordinate his salary out of her initial endowment of funds, which was held constant at 100 experimental dollars in each period. In the Public Superior Pay treatment, the level of the superior's initial endowment was common knowledge, as was the requirement that all 100 experimental dollars were spent on the subordinate's salary. Similar to prior experimental budgeting studies, this treatment made relative pay between the superior and the subordinate transparent and created a budgeting setting where the salience of distributional fairness was increased. In the Private Superior Pay treatment, only the superior knew the level of her initial endowment, which was still fixed at 100 experimental dollars. It was common knowledge that the superior had to use 100 experimental dollars of her endowment to pay the subordinate's salary. Because the superior's initial endowment was known only by the superior, the subordinate could not evaluate relative pay outcomes definitely. Thus, the salience of distributional fairness was decreased in this setting.

Consistent with RSY, we manipulate the mode of budget communication to capture the incremental effect of honesty on budgetary slack. In the Factual Assertion treatment, the

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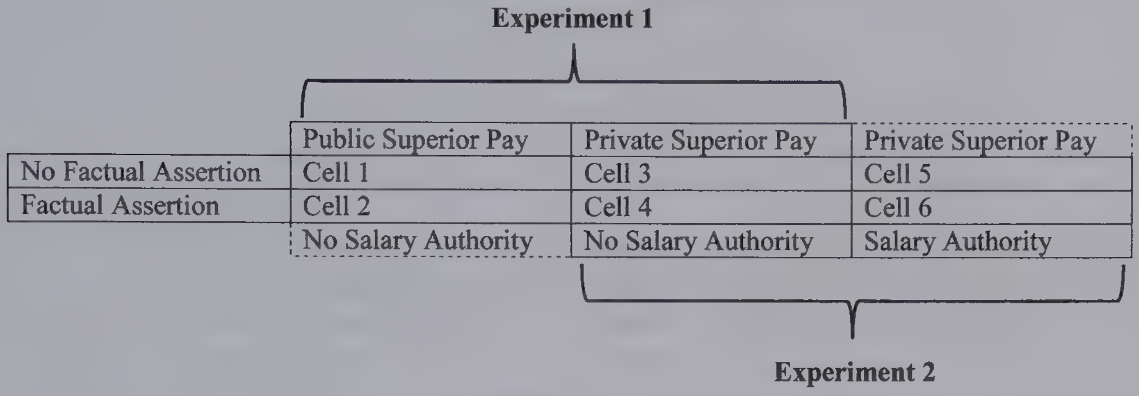
[9] The two roles were labeled "owner" and "manager" in the experimental instructions. For consistency, the terms superior and subordinate are used in the continued exposition.

[10] The level of the superior's endowment was set to create an economically identical setting to the Superior Authority treatment in RSY, with the exception that the subordinate's salary came from the superior's endowment rather than directly from the experimenter.

FIGURE 1
Experimental Design of Experiments 1 and 2



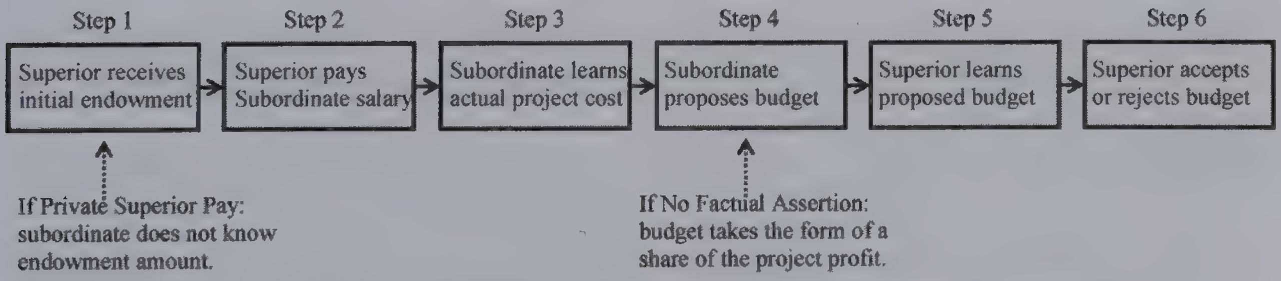
	Public Superior Pay	Private Superior Pay	Private Superior Pay
No Factual Assertion	Cell 1	Cell 3	Cell 5
Factual Assertion	Cell 2	Cell 4	Cell 6
	No Salary Authority	No Salary Authority	Salary Authority

Experiment 1 examines the robustness of honesty effects on budgetary slack to distributional fairness norms by manipulating whether the relative pay (initial endowment) of the superior is public or private. Experiment 2 examines the robustness of honesty effects on budgetary slack to reciprocity norms by manipulating whether the superior has the authority to set the subordinate's salary. The incremental effect of honesty on budgetary slack is measured by manipulating whether the budget communication includes a factual assertion. Both experiments utilize a 2 × 2 experimental design and share the same two control cells (Cells 3 and 4).

subordinate's budget required an assertion of fact regarding the project's actual cost. Since there was the possibility of making an untrue assertion or dishonest budget, this treatment created a setting where the salience of honesty was increased. In the No Factual Assertion treatment, the subordinate's budget proposed the amount of profit from the project going to the superior. Since there was no possibility of making an untrue assertion, this treatment created a budgeting setting where the salience of honesty was decreased. In both treatments, the subordinate was not allowed to achieve a negative outcome from the project (i.e., underreport actual costs or offer more than the full profit). All of our experimental treatments provide the same agency prediction (Camerer 2003). That is, the subordinate is predicted to report the highest possible cost or offer the lowest possible profit from the project and the superior is predicted to always accept the project to maximize her wealth. The timeline for Experiment 1 is presented in Figure 2.

Participants were students at a large southeastern university who interacted anonymously through a computer network using the z-Tree software package (Fischbacher 2007). Prior to the experimental sessions, a set of randomly generated cost sequences was chosen and used in all treatments. This controlled for potential order effects and ensured that random noise in the actual costs did not drive participant behavior differently between treatments. This procedure also ensured

FIGURE 2
Timeline for Experiment 1


Step 1
Superior receives initial endowment
Step 2
Superior pays Subordinate salary
Step 3
Subordinate learns actual project cost
Step 4
Subordinate proposes budget
Step 5
Superior learns proposed budget
Step 6
Superior accepts or rejects budget
If Private Superior Pay: subordinate does not know endowment amount.
If No Factual Assertion: budget takes the form of a share of the project profit.

After Step 6, managers and owners are randomly rematched and steps 1–6 repeat for a total of 20 periods.

that the realized cost was equal for each period across treatments. At the end of each session, participants were paid privately in cash. Participants received a show-up fee of $10 and earnings based upon their decisions in one randomly selected period at a rate of $1 = 5 experimental dollars.[11] The experiment was conducted in eight sessions, with 14 participants in half of the sessions and 16 participants in the rest of the sessions. Each session lasted about 45 minutes, and the average earnings was $20.13, in addition to the $10 show-up fee.

Results for Experiment 1

Participants responded to a number of statements on the exit questionnaire that were designed to test the effectiveness of our experimental manipulations and controls. Participants responded to these statements on a seven-point Likert scale (1 = Strongly Disagree, 7 = Strongly Agree). Tests of manipulations and controls are the mean difference from the neutral response of 4. In untabulated results, we find evidence that our experimental manipulations and controls were effective for Experiment 1 (all $p < 0.05$). To examine if we successfully manipulated the salience of distributional fairness, we examine responses to the exit questionnaire item, "I wanted both parties to have even payoffs." This is a measure of the subordinate's concern for the equal-split norm, which is a distributional fairness norm that frequently becomes a focal point in ultimatum games when relative pay is transparent (Fehr and Schmidt 1999; Camerer 2003; Bicchieri 2006; Andreoni and Bernheim 2009). Responses in the Private Superior Pay treatment were significantly lower for this item than those in the Public Superior Pay treatment (3.4 versus 4.7, $t = 4.01$, $p < 0.01$). Thus, our attempt to manipulate the salience of distributional fairness by withholding relative pay information from the subordinate appears to have been successful.

The descriptive statistics for Experiment 1 are presented in Table 1.[12] As in prior research, we measure budgetary slack as the slack claimed over the slack available (Evans et al. 2001; RSY 2008). Since the realized cost is the same in each decision period across treatments, the total slack available over the course of the experiment is the same in each treatment and the average slack claimed by subordinates can be compared easily between treatments and over time. The average cost in each treatment is 100.4 experimental dollars. In Experiment 1, the highest level of budgetary slack occurred in the Private Superior Pay/No Factual Assertion treatment, followed by the Public Superior Pay/No Factual Assertion treatment, then the Public Superior Pay/Factual Assertion treatment, and slack was lowest in the Private Superior Pay/Factual Assertion treatment. These mean results are graphed in Figure 3.

Our first hypothesis predicts that honesty will have a greater effect on budgetary slack when the relative pay of the superior is withheld from the subordinate. To test this prediction, we estimate an ANOVA on the effect of *Factual Assertion, Private Superior Pay,* and the interaction of these two dummy variables on budgetary slack.[13] The results of this analysis are reported in Panel A of Table 2. In the full model, we document a strong main effect on *Factual Assertion* ($F = 18.13$, $p < 0.01$,

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[11] Randomly rematching superior/subordinate pairs each period and paying participants for only one randomly selected period helped ensure that our experiments formed a repeating, single-period setting. This was important to control for reputation and other social effects that were not the focus of our study and to increase the theoretical link with Antle and Eppen's (1985) single-period model.

[12] Similar to prior studies with superior rejection authority (e.g., Schatzberg and Stevens 2008; RSY 2008), all of our descriptive statistics and statistical analyses include data from periods where the subordinate's budget was rejected.

[13] To control for the lack of independence caused by each participant making multiple decisions, the average level of slack over all decision periods is recorded for each participant and treated as a single observation for all our hypothesis tests utilizing ANOVAs. We estimate a repeated-measure ANOVA to test for *Period* effects. We find a significant main effect on *Period,* but no interaction effect of *Period* and our between-subject variables. Slack is lower in the first five periods than in the last 15 periods, which is driven by low slack in the first two periods. Our results are qualitatively unchanged when excluding the first five periods or analyzing the first five periods alone.

TABLE 1

Descriptive Statistics: Experiment 1

	Public Superior Pay		**Private Superior Pay**	
	Cell 1		Cell 3	
No Factual Assertion	Average % Slack	0.561	Average % Slack	0.614
	Std. Dev. of Slack	0.176	Std. Dev. of Slack	0.194
	Average Salary	100	Average Salary	100
	% Rejections	0.335	% Rejections	0.320
	Average Subordinate Profit	144.62	Average Subordinate Profit	148.87
	Average Superior Profit	34.97	Average Superior Profit	30.33
	Cell 2		Cell 4	
Factual Assertion	Average % Slack	0.487	Average % Slack	0.411
	Std. Dev. of Slack	0.248	Std. Dev. of Slack	0.262
	Average Salary	100	Average Salary	100
	% Rejections	0.224	% Rejections	0.290
	Average Subordinate Profit	139.44	Average Subordinate Profit	132.36
	Average Superior Profit	44.70	Average Superior Profit	49.10

In each cell, there were 15 participants in the role of subordinates and 15 participants in the role of superiors. The participants interacted for 20 periods in each cell.

Average % Slack is defined as slack claimed/slack available, and ranges from 0 to 1.

Average Salary is the average salary paid to the manager from the owner's endowment.

% Rejections is defined as budgets rejected/budgets proposed.

Average Subordinate Profit is defined as the average experimental dollars earned by subordinates from their salary and the project (whether accepted or not).

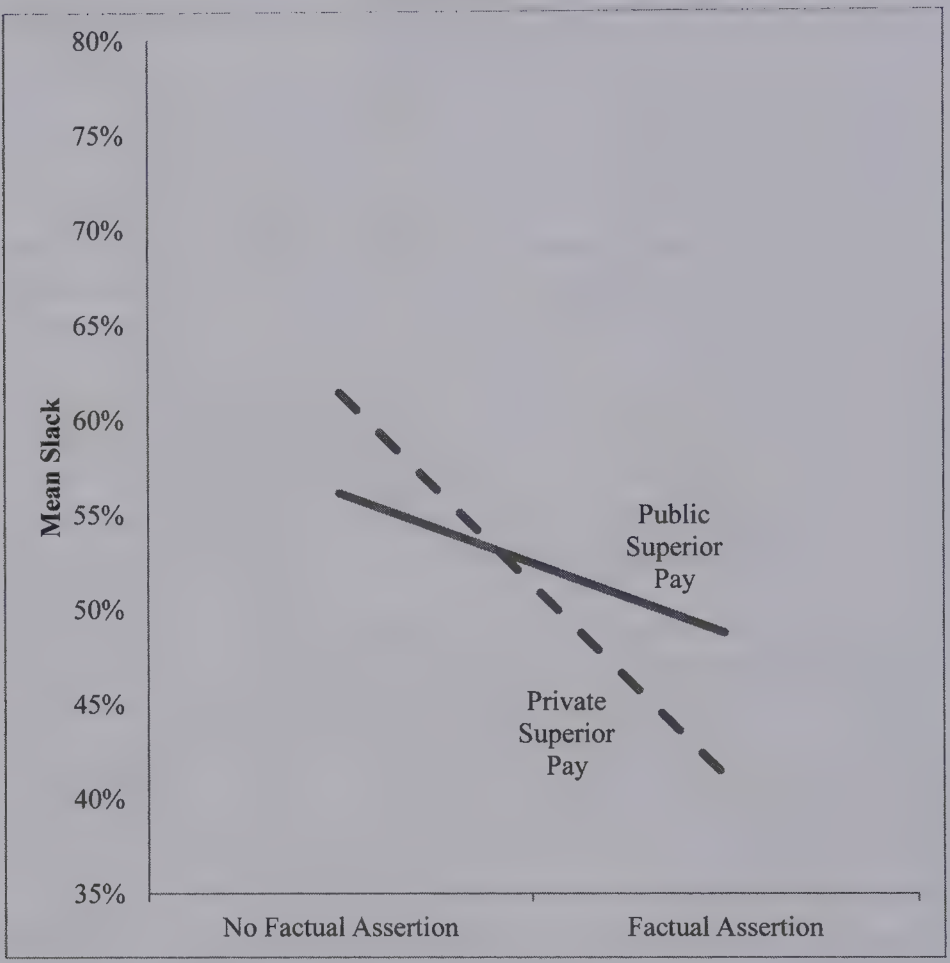
Average Superior Profit is defined as the average experimental dollars earned by superiors from the project (whether accepted or not).

The average cost of the project was 100.4 experimental dollars in each treatment.

two-tailed) and the hypothesized interactive effect on *Factual Assertion* and *Private Superior Pay* ($F = 3.95$, $p = 0.026$, one-tailed). As reflected in Figure 3, this interaction is in the direction predicted by H1. To provide further insights regarding this interaction, we analyze the simple main effects within treatments. The results of this analysis are presented in Panel B of Table 2. While *Factual Assertion* is only marginally significant in the Public Superior Pay treatment ($F = 3.01$, $p = 0.094$, two-tailed), it is highly significant in the Private Superior Pay treatment ($F = 17.06$, $p < 0.01$, two-tailed). The significance of the interaction between *Factual Assertion* and *Private Superior Pay* reflects that these two p-values are significantly different. This provides further support for H1.

Our finding of a marginally significant effect of honesty on budgetary slack under the Public Superior Pay treatment is inconsistent with RSY's finding of no effect for honesty under superior rejection authority. While our study is set in a similar strategic budgeting setting, there are some differences between our two studies that may explain this differential honesty result. Most importantly, the subordinate's salary came out of the superior's endowment in our experiment, whereas the subordinate's salary came directly from the experimenter in RSY. We argue that paying the subordinate's salary out of the superior's endowment captures an important aspect of budgeting settings in practice in that the resources of the firm are limited and the superior is thereby motivated to be efficient with those resources. This design choice was also useful in our experimental study to withhold relative pay information from subordinates in Experiment 1 and to manipulate the ability of the superior to set the subordinate's salary in Experiment 2. Although we

FIGURE 3
Mean Budgetary Slack across Experimental Conditions
Experiment 1



do not directly replicate RSY's no-effect result for honesty under superior authority, the interaction we document between distributional fairness and honesty provides new insights that may explain RSY's result. In particular, our interaction result suggests that the salience of distributional fairness in RSY's experimental setting may have reduced the salience of honesty in their study.

To provide more evidence of the ability of distributional fairness to diminish the incremental effect of honesty on budgetary slack, we include participants' responses to the exit questionnaire item that measured the subordinate's concern for even payoffs ("I wanted both parties to have even payoffs.") as a covariate in our full model. If the interaction term of *Factual Assertion* and *Private Superior Pay* is significantly weakened with this covariate in the model, then this would provide evidence that this interaction is driven by the salience of distributional fairness diminishing honesty effects in our experiment. The results of this ANCOVA analysis are presented in Panel C of Table 2. When included in the model as a covariate, the measure of the subordinate's concern for even payoffs (*Concerns for Even Payoffs*) is significant ($F = 2.37$, $p = 0.043$, two-tailed) and the interaction term *Factual Assertion* $*$ *Private Superior Pay* is no longer significant ($F = 0.78$, $p = 0.38$, two-tailed). The main effect for *Factual Assertion,* however, remains highly significant ($F = 14.37$, $p < 0.01$, two-tailed). This ANCOVA analysis supports our interpretation that the interaction we document between *Factual Assertion* and *Private Superior Pay* is due to distributional fairness diminishing the effect of honesty on budgetary slack.

TABLE 2

The Effect of Factual Assertion and Private Superior Pay on Budgetary Slack Experiment 1

Panel A: Analysis of Variance—Full Model

Factor	F	p-value[a]
Factual Assertion	18.13	<0.001
Private Superior Pay	0.13	0.725
*Factual Assertion * Private Superior Pay*	3.95	**0.026**

Panel B: Simple Main Effects

Factor	F	p-value
Factual Assertion within Public Superior Pay	3.01	0.094
Factual Assertion within Private Superior Pay	17.06	<0.001
Private Superior Pay within No Factual Assertion	3.30	0.080
Private Superior Pay within Factual Assertion	1.72	0.201

Panel C: Analysis of Covariance—Concern for Even Payoffs in the Full Model

Factor	F	p-value
Factual Assertion	14.37	<0.001
Private Superior Pay	0.000	0.951
*Factual Assertion * Private Superior Pay*	0.78	0.380
Concern for Even Payoffs (covariate)	2.37	0.043

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[a] All p-values are two-tailed except for those relating to directional predictions, which are one-tailed and shown in bold. The dependent variable in this analysis is the mean budgetary slack (*Slack*) for each subordinate over all decision periods.

Variable Definitions:
Slack = each period is calculated as slack claimed/slack available;
Factual Assertion = a dummy variable capturing the mode of budget communication, equaling 1 if the budget report required a factual assertion, and 0 otherwise;
Private Superior Pay = a dummy variable capturing the subordinate's knowledge about the superior's endowment, equal to 0 if the subordinate knew the endowment, and 1 otherwise;
*Factual Assertion * Private Superior Pay* = interaction term of *Factual Assertion* and *Private Superior Pay;* and
Concern for Even Payoffs = measured by the subordinate's response to an exit questionnaire about their agreement with the statement "I wanted both parties to have even payoffs," on a seven-point Likert scale.

IV. EXPERIMENT 2: HONESTY AND RECIPROCITY

Hypothesis Development for Experiment 2

In Experiment 2, we examine the robustness of honesty effects on budget proposals when the superior has rejection authority by manipulating the salience of reciprocity. Reciprocity is generally defined as repaying intentional actions in like kind, such as repaying kind actions with kindness and unkind actions with unkindness (Rabin 1993; Cox and Deck 2005). Thus, reciprocity requires an intentional act by the first-mover, the nature of which is recognizable by the second-mover so that they may respond in like kind (Fehr and Gachter 2000; Bellemare, Kroger, and van Soest 2011). The effects of reciprocity in labor contracting and as a solution to agency problems have been widely studied in economics and accounting. Experimental researchers have documented "gift exchange" behavior whereby superiors pay subordinates above-market wages in exchange for above-minimal effort (Akerlof 1982; Fehr, Kirchsteiger, and Riedl 1993; Hannan 2005; Kuang and Moser 2009; Douthit et al. 2012). Despite the abundance of experimental studies of reciprocity in contracting settings, researchers in accounting have only recently begun to examine reciprocity in a budgeting setting (Schatzberg and Stevens 2008; Zhang 2008).

We argue that honesty and reciprocity represent complementary social norms in a participative budgeting setting. This view is supported by experimental evidence and the supportive nature of honesty and reciprocity norms in a participative budgeting setting. Takahashi et al. (2012) combine personality inventories, economic game theory, and neurobiology to test the relation between honesty and reciprocity norms. They document that honest individuals are more likely to engage in the costly act of reciprocity. Their results suggest that subordinates who are predisposed to the activation of honesty in a budgeting setting are similarly predisposed to the activation of reciprocity. This suggests that increasing the salience of reciprocity norms will not necessarily diminish honesty effects on budgetary slack. Further, behavior that is consistent with a reciprocity norm in a participative budgeting setting is for the subordinate to increase the honesty of the budget after receiving a higher salary from the superior. This behavior supports the honesty norm, and suggests that increasing the salience of reciprocity will support honesty in a budgeting setting.

As discussed above, Bicchieri's (2006, 131) model predicts that where multiple social norms are present, expectations and behavior will be affected by the most salient of the social norms. As situational cues make reciprocity more salient, it increases the probability that this social norm will be activated. In contrast to distributional fairness, however, this need not diminish honesty effects, since reciprocity and honesty norms do not make conflicting prescriptions in our experimental setting. Further, Bicchieri's (2006, 70) model predicts that social norms can be elicited indirectly by focusing on other, conceptually similar norms. We expect that giving the superior the authority to set the salary of the subordinate is a powerful situational cue that makes reciprocity salient. Thus, we manipulate the salience of reciprocity norms by manipulating the salary authority of the superior, and we expect honesty effects on budgetary slack to be robust to the presence of this authority. Based on Bicchieri's (2006) model and experimental evidence, in H2, we predict that honesty will have an effect on budgetary slack when the superior sets the salary of the subordinate:

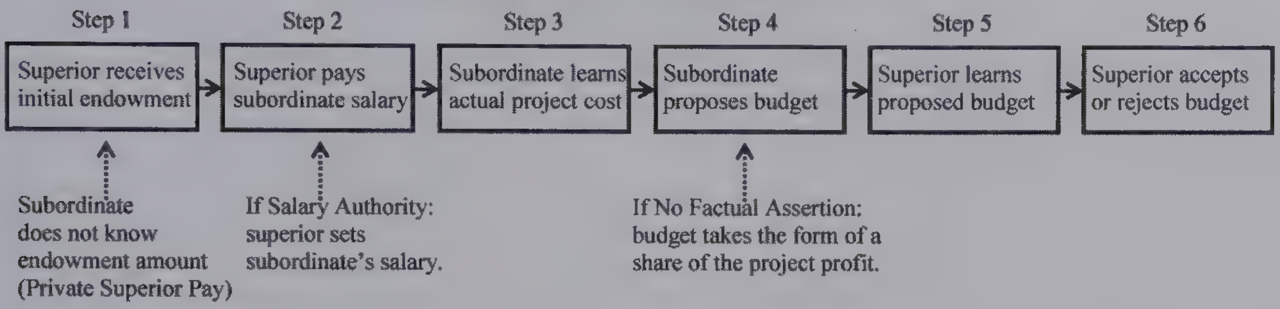
> **H2:** Honesty will have an effect on budgetary slack when the superior sets the salary of the subordinate.

Experimental Design for Experiment 2

To test our second hypothesis, we extend the Private Superior Pay treatment from Experiment 1, where the salience of distributional fairness is decreased, by manipulating the salience of reciprocity. In particular, we utilize a 2 × 2 factorial design where the authority of the superior to set the subordinate's salary is manipulated at two levels (No Salary Authority versus Salary Authority) and the mode of budget communication is manipulated at two levels (Factual Assertion versus No Factual Assertion). The first manipulation allows us to examine the robustness of honesty effects to the salience of reciprocity norms, and the second manipulation allows us to capture the incremental effect of honesty on budgetary slack.

The timeline for Experiment 2 is presented in Figure 4. As in Experiment 1, the superior received an initial endowment of 100 experimental dollars, out of which she paid the subordinate's salary. While the presence of the endowment was common knowledge, the amount of the endowment was known only by the superior. Under the No Salary Authority treatment, the superior had to pay the subordinate a salary of 100 experimental dollars each period, as in Experiment 1. As such, this treatment created an experimental budgeting setting where the salience of reciprocity

FIGURE 4
Timeline for Experiment 2


Step 1
Superior receives initial endowment
Step 2
Superior pays subordinate salary
Step 3
Subordinate learns actual project cost
Step 4
Subordinate proposes budget
Step 5
Superior learns proposed budget
Step 6
Superior accepts or rejects budget
Subordinate does not know endowment amount (Private Superior Pay)
If Salary Authority: superior sets subordinate's salary.
If No Factual Assertion: budget takes the form of a share of the project profit.

After Step 6, managers and owners are randomly rematched and steps 1–6 repeat for a total of 20 periods.

norms was reduced. Under the Salary Authority condition, it was common knowledge that the superior chose the salary paid to the subordinate out of the superior's endowment each period. Prior to learning the actual cost of the project, the subordinate was informed of the amount of the salary set by the superior he was paired with that period. Any amount of the endowment not used to pay the subordinate's salary was kept by the superior. Since the superior made an intentional choice that could be perceived by the subordinate as more or less kind, this treatment created an experimental budgeting setting in which the salience of reciprocity was increased. All other aspects of Experiment 2 were identical to Experiment 1.

In the experimental treatment in which the superior does not have the authority to set the subordinate's salary (No Salary Authority), the same strong economic predictions as in Experiment 1 hold based on traditional game theory (Camerer 2003). That is, the subordinate is predicted to report the highest possible cost or offer the superior the lowest possible profit from the project, and the superior is predicted to always accept the project in order to maximize her wealth. In the experimental treatment in which the superior has salary authority, however, the superior is predicted to pay the subordinate a salary of zero and keep the entire endowment for herself because she can pay less than her full endowment and she expects to receive the minimal share of the project profit from the subordinate. Since the No Salary Authority treatment in Experiment 2 is exactly the same as the Private Superior Pay treatment from Experiment 1, we only collect additional data for the Salary Authority condition for Experiment 2 (see Figure 1). Experiment 2 was conducted in eight sessions, with 14 participants in half of the sessions and 16 participants in the rest of the sessions. Each session lasted about 45 minutes, and the average earnings per participant were $18.95, in addition to the $10 show-up fee.

Results for Experiment 2

As with Experiment 1, participants responded to a number of statements on the exit questionnaire that were designed to test the effectiveness of our experimental manipulations and controls using a seven-point Likert scale (1 = Strongly Disagree, 7 = Strongly Agree). Tests of manipulations and controls are the mean difference from the neutral response of 4. In untabulated results, we find evidence that our experimental manipulations and controls were effective for Experiment 2 (all $p < 0.05$). In particular, keeping the relative pay of the superior private reduced the salience of distributional fairness in all four experimental cells, and manipulating the ability of the superior to set the subordinate's salary increased the salience of reciprocity.

Descriptive statistics for Experiment 2 are presented in Table 3. Slack is the highest in the Salary Authority/No Factual Assertion treatment, followed by the No Salary Authority/No Factual

TABLE 3

Descriptive Statistics: Experiment 2

	No Salary Authority		Salary Authority	
	Cell 3		Cell 5	
No Factual Assertion	Average % Slack	0.614	Average % Slack	0.656
	Std. Dev. of Slack	0.194	Std. Dev. of Slack	0.246
	Average Salary	100	Average Salary	30.50
	% Rejections	0.320	% Rejections	0.183
	Average Subordinate Profit	148.87	Average Subordinate Profit	88.48
	Average Superior Profit	30.33	Average Superior Profit	101.38
	Cell 4		Cell 6	
Factual Assertion	Average % Slack	0.411	Average % Slack	0.513
	Std. Dev. of Slack	0.262	Std. Dev. of Slack	0.256
	Average Salary	100	Average Salary	30.37
	% Rejections	0.290	% Rejections	0.233
	Average Subordinate Profit	132.36	Average Subordinate Profit	72.96
	Average Superior Profit	49.10	Average Superior Profit	113.43

In each cell, there were 15 participants in the role of subordinates and 15 participants in the role of superiors. The participants interacted for 20 periods in each cell.
Average % Slack is defined as slack claimed/slack available, and ranges from 0 to 1.
Average Salary is the average salary paid to the subordinate from the superior's endowment.
% Rejections is defined as budgets rejected/budgets proposed.
Average Subordinate Profit is defined as the average experimental dollars earned by subordinates from their salary and the project (whether accepted or not).
Average Superior Profit is defined as the average experimental dollars earned by superiors from their residual endowment (in Cells 5 and 6) and the project (whether accepted or not).
The average cost of the project was 100.4 experimental dollars in each treatment.

Assertion treatment, the Salary Authority/Factual Assertion treatment, and then the No Salary Authority/Factual Assertion treatment. These means are graphed in Figure 5.

In H2, we predict that honesty will have an effect on budgetary slack when the superior sets the salary of the subordinate. To test this prediction, we estimate an ordinary least squares (OLS) regression of the effect of the factual assertion (*Factual Assertion*), the salary set by the superior (*Salary*), and the interaction of these two factors on budgetary slack while clustering the error terms of each subordinate to control for the repeated-measures nature of responses. To find the effect of the mode of budget communication at the average salary and the average effect of the salary level across the two modes of budget communication, we center *Factual Assertion* and *Salary* so the mean value of these unadjusted variables is loaded into the regression as 0.[14]

We report the results of this analysis in Panel A of Table 4. Consistent with Zhang (2008), subordinates in our study reciprocate higher salaries from the superior by claiming lower slack, as evidenced by a negative coefficient on *Salary* ($t = -7.94$, $p < 0.01$, one-tailed). More importantly, consistent with H2, we find that honesty effects on budgetary slack are robust to the salience of

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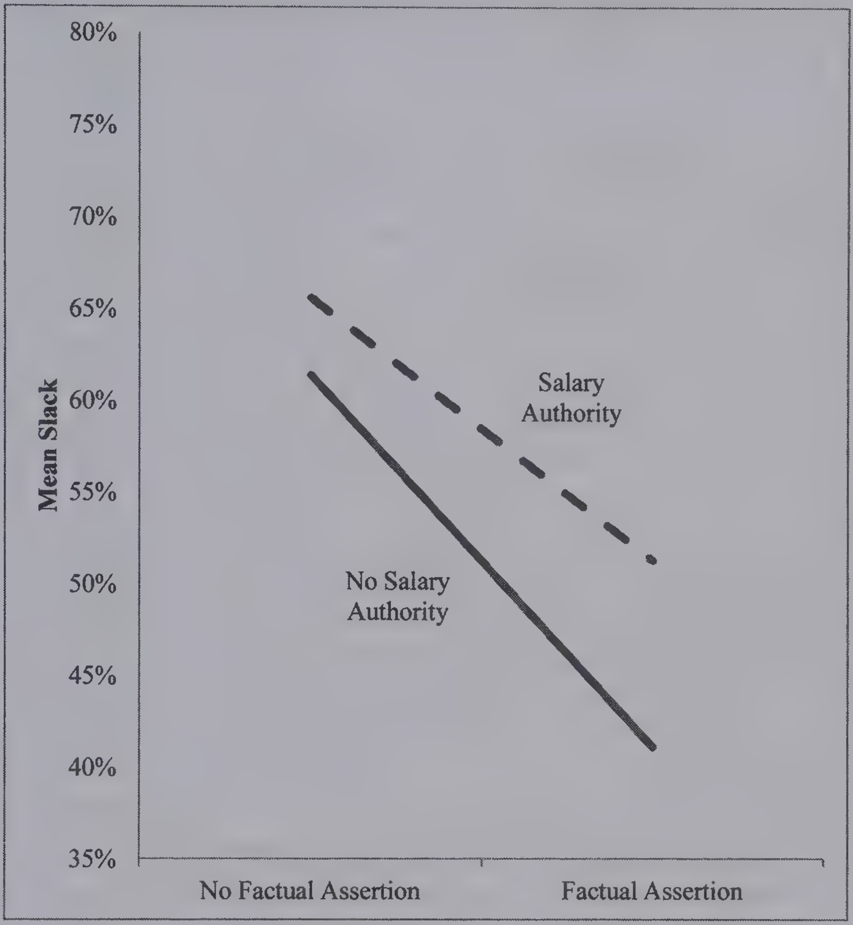
[14] This is accomplished by subtracting the unadjusted mean for each variable from each observation so that *Salary* and *Factual Assertion* have a mean of zero. As such, the main effect of *Salary* can be interpreted as the effect of salary across factual assertion treatment, and the main effect of *Factual Assertion* can be interpreted as the effect of the factual assertion manipulation at the average salary (i.e., when *Salary* equals zero).

FIGURE 5
Mean Budgetary Slack across Experimental Conditions
Experiment 2



reciprocity norms. This is evidenced by an insignificant interaction between *Factual Assertion* and *Salary* ($t = 0.13$, $p > 0.10$) and a negative main effect for *Factual Assertion* ($t = -2.74$, $p < 0.01$, one-tailed). Across salary levels, budgetary slack is significantly lower when the budget communication included a factual assertion than when it did not.

To provide further evidence regarding H2, we estimate an ANOVA on the effect of *Factual Assertion, Salary Authority,* and the interaction of these two dummy variables on budgetary slack. The results of this analysis are presented in Panel B of Table 4. We find a significant main effect for *Factual Assertion* ($F = 17.46$, $p < 0.01$) and *Salary Authority* ($F = 6.35$, $p = 0.02$, two-tailed), but an insignificant interaction between *Factual Assertion* and *Salary Authority* ($F = 0.51$, $p = 0.48$, two-tailed). This evidence suggests that the salience of reciprocity does not diminish the incremental effect of honesty on budget proposals. To provide more evidence for the robustness of honesty effects on budgetary slack when the salience of reciprocity is manipulated, we present the simple main effects in Panel C of Table 4. We find a highly significant main effect for *Factual Assertion* both when the superior has salary authority ($F = 6.26$, $p < 0.01$, one-tailed) and when the superior does not have salary authority ($F = 17.06$, $p < 0.01$). In summary, we find strong and consistent support for H2.

As explained above, traditional game theory predicts that the superior would set the salary of the subordinate at zero under salary authority because this allows her to keep her full endowment and she expects to receive the minimal share of the project profit from the subordinate. In contrast to

TABLE 4

The Effect of Factual Assertion and Salary Authority on Budgetary Slack: Experiment 2

Panel A: OLS Regression of Budgetary Slack with Salary Authority Effects

	Coefficient (t-stat)
Factual Assertion	−0.124 (−2.74)***
Salary	−0.004 (−7.94)***
*Factual Assertion * Salary*	0.000 (0.13)

$R^2 = 0.1802$

*** Represents significance at the 1 percent level.

Panel B: Analysis of Variance—Full Model

Factor	F	p-value[a]
Factual Assertion	17.46	<0.001
Salary Authority	6.35	0.015
*Factual Assertion * Salary Authority*	0.51	0.479

Panel C: Simple Main Effects

Factor	F	p-value
Factual Assertion within No Salary Authority	17.06	<0.001
Factual Assertion within Salary Authority	6.26	**0.009**
Salary Authority within No Factual Assertion	0.60	0.446
Salary Authority within Factual Assertion	3.87	0.059

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[a] All p-values are two-tailed except for those relating to directional predictions, which are one-tailed and presented in bold.

The dependent variable in these analyses is *Slack,* which is the mean slack for each subordinate over all decision periods.

Variable Definitions:
Slack = each period is calculated as slack claimed/slack available;
Factual Assertion = a dummy variable for the mode of budget communication, equal to 1 if the budget report required a factual assertion, and 0 otherwise, centered at the mean of the dummy variable;
Salary = salary set by the superior for the subordinate, centered at the mean of salary set;
Salary Authority = a dummy variable for the superior's ability to set the subordinate's salary, equal to 1 if the superior set the subordinate's salary, and 0 otherwise;
*Factual Assertion * Salary* = interaction term of *Factual Assertion* and *Salary;* and
*Factual Assertion * Salary Authority* = interaction term of *Factual Assertion* and *Salary Authority.*

this stark economic prediction, however, the average salary set by the superior under salary authority is 30 (see Table 3). The average salary appears generous in view of traditional game theory, but is well below the required salary of 100 when the superior did not have salary authority. Because the average salary is above zero, it provides evidence that superiors expected the subordinate to reciprocate higher salary levels by building less budgetary slack. Because the average salary is below 100, it helps explain why budgetary slack levels were higher, on average, when the superior had salary authority.

Supplemental Analyses of Superior Rejection Behavior in Experiment 2

As discussed previously, Bicchieri's (2006) model suggests that a social norm is activated and followed when individuals become aware that a given norm is relevant to the current social setting and a combination of empirical and normative expectations gives individuals a sufficient reason to follow the norm. Normative expectations are based on the belief that a sufficiently large subset of people expects conformance to the norm in similar settings and may be willing to sanction behavior inconsistent with the norm. Therefore, normative expectations and the potential sanctioning behavior of the referent group play an important role in social norm activation. This suggests that social norms activated by the budgeting setting will affect the expectations and behavior of the superior, in addition to the subordinate. Given that the superior could sanction the behavior of the subordinate by rejecting the budget, the rejection behavior of the superior could play an important role in supporting honesty and reciprocity norms in the subordinate. Therefore, based on Bicchieri's (2006) model, we expect superiors to be more likely to reject budget proposals that contain high levels of budgetary slack when social norms for honesty or reciprocity have been activated, controlling for the level of the budget reported.

To test the effect of honesty and reciprocity norms on superior rejection behavior, we use a logistic regression of the effects of the mode of budget communication (*Factual Assertion*), the salary chosen for the subordinate (*Salary*), and the generosity of the budget report (*Generosity*) on the rejection decision when the superior has Salary Authority. *Generosity* is the amount of money the superior would receive from the project if she accepted the subordinate's report. The error terms are clustered at the participant level to control for the repeated-measures nature of responses. The results of this analysis are presented in Table 5. *Generosity* is a significant predictor of rejection behavior ($z = -6.76$, $p < 0.01$). Nevertheless, the coefficient on *Factual Assertion* is positive and significant ($z = 2.19$, $p = 0.014$, one-tailed), as is the coefficient on *Salary* ($z = 2.41$, $p < 0.01$, one-tailed). Thus, controlling for the generosity of the subordinate's budget request, the activation of honesty and reciprocity norms appeared to increase the likelihood that the superior would reject the budget.[15] We conclude, consistent with Bicchieri's (2006) model, that activated social norms for honesty and reciprocity affected the expectations and behavior of the superior, in addition to the subordinate.

Supplemental Analyses of Firm Profit in Experiment 2

From an agency theory perspective, the main importance of honesty and reciprocity norms to participative budgeting is their effectiveness at mitigating reductions in the profitability of the principal-agent relation due to information asymmetry and economic self-interest. As such, it is important to test whether such social norms lead to higher profits for the superior. To examine the effect of honesty and reciprocity on superior profit, we estimate an ANOVA of the effect of *Factual Assertion, Salary Authority,* and the interaction of these two dummy variables on the superior's earnings. As with our ANOVAs analyzing budgetary slack, we control for the lack of independence in participants' multiple responses by averaging earnings for each superior over all periods and treating this mean as a single observation. The results of this analysis are presented in Panel A of Table 6. We find that both main effects have a significant effect on the superior's payoff. In particular, superior earnings are higher when the budget communication required a factual assertion ($F = 24.07$, $p < 0.01$) and when the superior had salary authority ($F = 464.00$, $p < 0.01$). The significant effect of *Factual Assertion* on firm profit suggests that honesty increased the profitability of the principal-agent

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[15] We investigated the two-way and three-way interaction of these factors and found no interaction between them. The inclusion of these interaction effects does not qualitatively change our results. In untabulated results, we find a significant effect of the factual assertion in the No Salary Authority treatment ($z = 1.87$, $p = 0.03$, one-tailed).

TABLE 5

Results of Logit Regression of Factual Assertion, Salary, and Reporting Generosity on Rejection Decisions of the Superior: Experiment 2

	Coefficient (z-stat)
Factual Assertion	0.871 (2.19)**
Salary	0.019 (2.41)**
Generosity	−0.064 (−6.76)***
Log pseudo-likelihood = −233.509	

, * Represent two-tailed significance at the 5 percent and 1 percent levels, respectively.
The dependent variable in this regression is *Reject*. The error terms for this regression are clustered at the subject level.

Variable Definitions:
Reject = a dichotomous variable equal to 1 when a project is rejected by the superior, and 0 otherwise;
Factual Assertion = a dummy variable for the mode of budget communication manipulation, equal to 1 if the budget report required a factual assertion, and 0 otherwise;
Salary = salary set by the superior for the subordinate each period; and
Generosity = level of profit from the project that would be realized by the superior if the project was accepted.

relation for the superior, consistent with honesty mitigating agency problems in this setting. The significant effect of *Salary Authority* on firm profit suggests that superiors had a somewhat accurate expectation about the level of reciprocity to expect from subordinates. The *Factual Assertion* $*$ *Salary Authority* interaction term is insignificant ($F = 1.14$, $p > 0.10$) and the simple main effects in Panel B of Table 6 verify the robustness of the honesty and reciprocity effects on firm profit.

Given the significant effect of *Salary Authority* on superior profit, we further examine the determinants of superior profit in the Salary Authority treatment. In particular, we estimate a clustered OLS regression of the effect of the factual assertion (*Factual Assertion*), linear and nonlinear effects of salary (*Salary* and $Salary^2$, respectively), and the interactive effects of the factual assertion with the linear and nonlinear salary effects in the Salary Authority treatments on the superior's earnings.[16] As before, *Factual Assertion* and *Salary* are centered so the mean value of the unadjusted variables is equal to zero and the higher order terms are the interactions of the centered variables. The results of this regression are presented in Panel C of Table 6. We find that *Salary* is negatively related to superior profit ($t = -15.55$, $p < 0.01$), but this negative relationship becomes less negative as the salary increases, as reflected in the significantly positive coefficient on $Salary^2$ ($t = 3.32$, $p < 0.01$). Thus, while increasing the subordinate's salary decreased the level of budgetary slack, it did so at a rate of less than one-to-one. Superiors appear to have partially anticipated the reciprocal response to salaries and improved their payoffs by choosing positive, but low, salaries for subordinates. However, the average response of the subordinate to salary changes was sufficiently small that the profit-maximizing salary was zero. As such, superiors were only partially successful at reaping higher profits through reciprocity when they could set the subordinate's salary. Collectively, our results suggest that honesty continued to play an important role in mitigating agency problems in the presence of reciprocity.

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[16] We test for nonlinear effects of the salary since prior literature on gift exchange-type relationships have found nonlinearity in the response to different salary levels (e.g., Hannan 2005).

TABLE 6

The Effect of Budget Communication and Salary Authority on Firm Profit: Experiment 2

Panel A: Analysis of Variance—Full Model

Factor	F	p-value
Factual Assertion	24.07	<0.001
Salary Authority	464.00	<0.001
Factual Assertion ∗ Salary Authority	1.14	0.291

Panel B: Simple Main Effects

Factor	F	p-value
Factual Assertion within No Salary Authority	27.91	<0.001
Factual Assertion within Salary Authority	5.42	0.027
Salary Authority within No Factual Assertion	423.63	<0.001
Salary Authority within Factual Assertion	150.06	<0.001

Panel C: Clustered OLS Regression of Firm Profit with Salary Authority Effects

	Coefficient (t-stat)
Factual Assertion	12.854
	(4.04)***
Salary	−7.050
	(−15.55)***
Salary^2	0.006
	(3.32)***
Factual Assertion ∗ Salary	0.062
	(0.68)
Factual Assertion ∗ Salary^2	−0.001
	(−0.29)

$R^2 = 0.1880$

*** Represents significance at the 1 percent level.
The dependent variable in these analyses is *Firm Profit*.

Variable Definitions:
Firm Profit = earnings for each superior over all decision periods;
Factual Assertion = a dummy variable for the mode of budget communication, equal to 1 if the budget report required a factual assertion, and 0 otherwise, centered at the mean of the dummy variable;
Salary = salary set by the superior for the subordinate, centered at the mean of salary set;
Salary^2 = interaction term of *Salary* and *Salary;*
Factual Assertion ∗ Salary = interaction term of *Factual Assertion* and *Salary;* and
Factual Assertion ∗ Salary^2 = interaction term of *Factual Assertion* and *Salary*^{2}.

V. CONCLUSION

Rankin et al. (2008; RSY) examine the effect of honesty on budget proposals in a participative budgeting setting by manipulating the mode of communication of the budget proposal, such that subordinates either provide a factual assertion of actual project cost or propose a division of the project profit without a factual assertion. They find no effect of honesty on budgetary slack when the superior can reject the subordinate's budget proposal. Using RSY's manipulation to capture honesty effects, we examine the robustness of honesty effects on budget proposals when the superior has rejection authority in two experiments. In Experiment 1, we document that honesty has a strong effect on budgetary slack when the salience of distributional fairness is reduced by withholding the relative pay of the superior from the subordinate. In Experiment 2, we document that honesty continues to have a strong effect on budgetary slack when the salience of reciprocity is increased by giving the superior the ability to set the subordinate's salary. Thus, our evidence suggests that honesty effects on budget proposals are generally robust to giving the superior rejection authority. In supplemental analysis of superior rejection behavior, we find that superiors were willing to engage in the costly behavior of rejecting the budget to enforce honesty and reciprocity norms in the subordinate's budget.

Our study follows the suggestions in Brown et al. (2009) regarding the conditions in which experiments have the greatest potential to extend traditional agency theory. We utilize a baseline prediction from the agency model of participative budgeting in Antle and Eppen (1985), and incorporate insights from Bicchieri's (2006) model of social norm activation to explain why our results are expected to deviate from that baseline prediction. We find Bicchieri's (2006) model useful in predicting how situational cues present in the budgeting setting can affect the salience of certain social norms, and how the salience of such norms interacts with the honesty norm in this setting. In particular, we find Bicchieri's (2006) model useful in providing an alternative explanation for RSY's result that honesty did not affect budgetary slack under superior rejection authority. Our theoretical development and experimental results offer a novel interpretation of RSY's main result based on competing social norms.

Our results suggest that it is premature to conclude from RSY's experimental study that honesty plays no role in strategic participative budgeting settings. While superiors typically have rejection authority, subordinates rarely know the relative pay of their superiors in practice, as they did in RSY. We document that honesty has a strong effect on budgetary slack when the subordinate does not know the relative pay of the superior. This increases our confidence that honesty plays an important role in participative budgeting in practice. Our confidence in the importance of honesty is further increased by our finding that the effect of honesty is robust to the salience of reciprocity when the superior sets the subordinate's salary. The authority to set subordinate pay is implicit in the hierarchical relationship between the superior and the subordinate in typical budgeting settings. Finally, our confidence in the importance of honesty is increased by our finding that the rejection behavior of the superior is also affected by honesty expectations in a way that enforces this social norm.

Our results support recent experimental evidence in accounting demonstrating that contextual cues can activate social norms that control opportunistic behavior across an increasing number of economic settings. For example, Tayler and Bloomfield (2011) show that social norms can control opportunism in the presence of formal controls, while Davidson and Stevens (2013) show that social norms can control opportunism through a manager's certification of a code of ethics. Our results also support the effort by some theorists to incorporate social norms into traditional agency theory. While the possibility for social norms to control opportunistic behavior was raised early in the development of agency theory (Demski and Feltham 1978, 346), theorists have only recently incorporated this possibility into their models. For example, Mittendorf (2006) incorporates honesty preferences into a hidden-information, capital-budgeting model and examines the implications of this disutility for the owner's choice of transfer payments and cost hurdles, while Stevens and Thevaranjan (2010) incorporate a morally sensitive agent in the traditional hidden-action, principal-agent model and examine the implications for the moral hazard problem. Consistent with these theoretical and experimental studies, our study suggests that adding social norms increases the descriptive usefulness of traditional agency theory.

As with prior experimental studies of participative budgeting, our study contains a stark experimental setting that is designed to provide a strong test of relevant theory. Although we implement a somewhat richer experimental setting than previous studies by giving the superior rejection and salary authority, our experimental setting still abstracts significantly from budgeting settings in practice. To the extent that our experimental design captures important aspects of these settings, however, we believe that our results provide useful insights that may generalize to such settings. Future research may provide additional insights by enriching the budgeting setting to include such factors as uncertainty in project cost, iterative bargaining, and repeated interactions between the superior and the subordinate. Our results suggest that future research is warranted to examine the main and interactive effects of social norms in participative budgeting settings. In particular, incorporating social norm theory may expand and enrich the "nexus of contracts" view of the firm in agency theory.

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APPENDIX A[17]

Instructions for Factual Assertion/No Salary Authority/Private Information

Introduction

Welcome and thank you for participating in this experiment. Your pay will depend on the decisions you make during the experiment. At the end of today's session, you will be paid in private and in cash. It is important that you remain silent and do not look at other people's work. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you.

Before the first period begins, participants will be assigned as owners or managers. Half of the participants will be assigned as owners and half of the participants will be assigned as managers.

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[17] Adapted from Rankin et al. (2008).

You remain as either an owner or manager for all periods. Each of you has an assigned subject number. *At the beginning of each period, subjects are randomly paired by subject numbers*. There will be 20 periods. Your experimental pay will be determined by as 20 times your earnings in one randomly drawn period. The period will be determined by the computer program at the conclusion of the experimental session.

Overview

Each period, each manager will be employed to implement a project for a fixed salary from each respective owner. Each period, the cost of implementing a project is randomly determined and revealed only to the manager. The cost is randomly drawn each period from the set of possible costs (0, 1, 2,. . .,200). These numbers represent pennies (i.e., 200 = $2.00). Each number is equally likely to be drawn each period. The manager learns the cost. The owner NEVER LEARNS THE COST. If implemented, then the project yields revenue of 200.

Owners' Task

At the beginning of the first period, the owner will learn what his or her initial level of funds will be, which does not vary between periods. Each period, the owner receives a cost report from the manager. The owner either rejects the project and gives the manager nothing, or accepts and funds the project and gets the 200 project revenue less the manager's reported cost, which is given to the manager.

Managers' Task

The manager will receive a fixed salary of 100 from the owner's initial funds each period regardless of the owner's decision. The manager observes the actual cost. After observing the actual cost, the manager reports a cost to the owner. The reported cost cannot be less than the actual cost. The owner either rejects the project and gives the manager nothing, or accepts the project and gets the 200 project revenue less the manager's reported cost. Since the project yields revenue of 200, the payoff to the owner is 200 minus the amount given to the manager if the project is implemented, and 0 otherwise. In addition, the owner will receive his or her initial funds each period less the salary of 100 paid to the manager each period, regardless of the owner's decision to implement the project. The payoff to the manager is the amount received from the owner minus the actual cost if the project is implemented, and 0 otherwise. In addition, the manager will receive the salary of 100 each period from the owner regardless of the owner's decision to implement the project.

Summary and Sequence of Events

The cost is randomly drawn each period from the set of possible costs (0, 1, 2,. . .,200). Each number is equally likely to be drawn each period. The manager learns the actual cost and submits a cost report to the owner. The reported cost must be equal to or greater than the actual cost. The owner either rejects the project and gives the manager nothing, or accepts the project and gives the manager an amount equal to the manager's reported cost. At the beginning of each period, subjects are randomly paired by subject numbers. There will be 20 periods.

Example

If the actual cost is 50, then the total profit is 200 – 50 = 150. The manager can report any cost between 50 and 200 to the owner. If the manager reports 133, then the owner either (1) rejects the report and the manager and owner receive nothing, or (2) accepts the report, gives 133 to the manager, and the owner earns $200 - 133 = 67$ plus his initial funds less the 100 paid to the manager, and the manager earns $100 + (133 - 50) = 183$. That is, if the report is rejected, then the manager and owner receive nothing from the project, although the manager still receives his salary of 100 and the owner still receives his initial funds less the manager's salary of 100. If the report is accepted, then the manager receives the salary of 100 plus the difference between the reported cost and actual cost $(133 - 50)$, and the owner receives the remainder of his initial funds (less the 100 paid to the manager) plus the profit above reported cost $(200 - 133)$.

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Does Internal Audit Function Quality Deter Management Misconduct?

Matthew S. Ege
University of Florida

ABSTRACT: Standard-setters believe high-quality internal audit functions (IAFs) serve as a key resource to audit committees for monitoring senior management. However, regulators do not enforce IAF quality or require disclosures relating to IAF quality, which is in stark contrast to regulatory requirements placed on boards, audit committees, and external auditors. Using proprietary data, I find that a composite measure of IAF quality is negatively associated with the likelihood of management misconduct even after controlling for board, audit committee, and external auditor quality. This result is robust to a variety of other specifications, including controlling for internal control quality and separate estimation during the pre- and post-SOX time periods. A difference-in-differences analysis indicates that misconduct firms have low IAF quality and competence during misconduct years and improve IAF quality and competence in the post-misconduct years. These findings suggest that regulators, audit committees, and other stakeholders should consider ways to improve IAF quality.

Keywords: *internal audit function; internal audit quality; management misconduct; corporate governance.*

I. INTRODUCTION

This study examines the relation between internal audit function (IAF) quality, as defined by standard-setters, and the likelihood of management misconduct, such as financial reporting fraud, bribery, and misleading disclosure practices. Standard-setters posit that IAFs serve as a key resource to audit committees for monitoring senior management and that high-quality IAFs deter management misconduct (American Institute of Certified Public Accountants [AICPA] 2005,

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2008; Committee of Sponsoring Organizations of the Treadway Commission [COSO] 2012; Institute of Internal Auditors [IIA] 2012). However, U.S. regulators do not enforce IAF quality or require disclosures relating to IAF quality.[1] NASDAQ recently proposed a new listing requirement that would have increased IAF quality, but then withdrew this proposal after receiving comments from the issuer community (Securities and Exchange Commission [SEC] 2013b). Commenting on the proposal, the CFO of Cytokinetics, Inc. stated, "[I]f we are trying to address underlying issues with management and board governance there are much better ways to do that than adding yet another compliance function" (SEC 2013c).

The treatment of the IAF is in stark contrast to the regulatory requirements placed on other corporate monitors of management, such as boards of directors, audit committees, and external auditors. In response to major accounting scandals such as Enron and WorldCom, regulators placed additional requirements on boards of directors, audit committees, and external auditors to improve the quality of each with respect to monitoring management. These requirements include a majority of independent directors, a fully independent audit committee, disclosure of audit committee financial expertise, and audit committee responsibility for hiring and firing the external auditor. Congress also established an external auditing regulator to enforce auditing standards and ensure audit quality. These requirements are intended to improve corporate governance and reduce the likelihood of major accounting scandals (SEC 2003c).

However, regulators did not impose requirements that directly improved IAF quality. The U.S. Securities and Exchange Commission (SEC) proposed requirements to increase IAF quality by adding the appointment, compensation, retention, and oversight of the internal auditor to audit committee responsibilities, but these proposed requirements were abandoned (SEC 2003b). Ten years later, after withdrawing its recent proposal to require high-quality IAFs, NASDAQ is considering how to revise the proposed rules (NASDAQ 2013). These proposals demonstrate the need for evidence regarding whether IAF quality results in improved monitoring of management. This study informs standard-setters, regulators, audit committees, and shareholders about whether IAF quality deters management misconduct incrementally to other monitors.

Using proprietary survey data from the Institute of Internal Auditors, I create a composite measure of IAF quality that captures experience, certification, training, use as a management training ground, reporting relationship with the audit committee, and size (Prawitt, Smith, and Wood 2009; Messier, Reynolds, Simon, and Wood 2011). I also employ a factor analysis to create measures of competence and objectivity, two components of IAF quality recognized by standard-setters (AICPA 1997; Public Company Accounting Oversight Board [PCAOB] 2010; International Auditing and Assurance Standards Board [IAASB] 2012; IIA 2012). Competence includes the knowledge and skills required to perform job responsibilities, while objectivity refers to an unbiased mental attitude toward audit matters (IIA 2012).

I then test whether IAF quality, competence, and objectivity are associated with observable instances of management misconduct, defined as credible allegations of intentional illegal acts by executive management, such as fraud and bribery. These data come from (1) SEC or Department of Justice enforcement actions claiming fraud, intentional misconduct, or violations of the bribery provision of the Foreign Corrupt Practices Act of 1977, and (2) settled securities class-action lawsuits. CEOs and CFOs are personally named in 75 percent of the misconduct cases included in the sample.

Based upon a final sample of 1,398 firm-years representing 617 unique firms from 2000 through 2009, I find a negative relation between IAF quality and management misconduct, even after controlling for other determinants of misconduct, including board of director, audit committee, and external auditor quality. This effect is economically significant, as a firm with IAF quality one standard deviation above the mean is approximately 2.3 percentage points less likely to have management misconduct than a firm with average IAF quality. This is approximately 29.5 percent of the 7.7 percent unconditional probability of management misconduct. Further analysis reveals that IAF competence, but not objectivity, is negatively related to the likelihood of management misconduct, suggesting that IAF competence is important in deterring management misconduct. Inferences remain the same after several robustness tests, including after controlling for internal control quality.

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[1] An exception is banking regulators, who require audit committees of U.S. banks to oversee the IAF and evaluate its performance and encourage directors to ensure the IAF complies with internal auditing standards (OCC 2003).

To help allay endogeneity concerns, I investigate how firms respond to revealed misconduct. If participants in the governance process believe IAF quality deters management misconduct, then misconduct firms should increase IAF quality in response to revealed misconduct. For example, after illegally marketing two drugs, Amgen agreed to establish a healthcare compliance internal audit program composed of an annual risk-based audit plan, audits performed by competent internal auditors, and oversight of corrective actions (Office of the Inspector General [OIG] 2012; Pollack and Secret 2012). Utilizing a difference-in-differences approach, I find that misconduct firms have low IAF quality and IAF competence during misconduct years as compared to a matched sample of firms. Then, in post-misconduct years, misconduct firms increase IAF quality through IAF competence. This increase in competence is due to hiring more certified internal auditors and increasing training. However, misconduct firms do not appear to have lower IAF objectivity during or after misconduct years compared to a matched sample. These results are consistent with the proposition from standard-setters that IAFs serve as a key resource for audit committees in monitoring management.

In additional analysis, I examine the effect of IAF quality on accounting- and nonaccounting-related misconduct separately. My findings suggest that high-quality IAFs are effective at deterring both types of management misconduct. This finding is particularly important because whereas external auditors focus on accounting-related management misconduct, internal auditors provide assurance related to both accounting- and nonaccounting-related management misconduct (Chadwick 2000; COSO 2012).

Next, I investigate the ability of IAF quality to predict accounting-related management misconduct after controlling for observable indicators of accounting fraud (Dechow, Ge, Larson, and Sloan 2011). External stakeholders such as investors generally lack information related to the IAF. Survey and experimental research report that disclosure of information related to the IAF would increase stakeholder confidence in governance quality and potentially affect investment decisions (Archambeault, DeZoort, and Holt 2008; Holt and DeZoort 2009). I find that IAF quality is negatively associated with accounting-related management misconduct even after controlling for observable predictors of accounting-related management misconduct. This suggests that disclosures related to IAF quality would assist stakeholders in predicting accounting-related management misconduct.

This study contributes to the literature by providing empirical evidence consistent with the proposition that IAF quality and competence deter management misconduct. Prior studies suggest that the existence of an IAF could deter management misconduct (e.g., Beasley, Carcello, Hermanson, and Lapides 2000), and there is mixed evidence as to whether IAF quality is associated with a commercially developed fraud risk measure (e.g., Christ, Masli, Sharp, and Wood 2012; Prawitt, Sharp, and Wood 2012). I extend this literature by providing evidence that suggests IAF quality and, particularly, IAF competence are important in deterring observable instances of management misconduct, both accounting- and nonaccounting-related. These findings are important because in the early 2000s, regulators responded to public outcry over observable management misconduct, yet IAF quality was largely left out of the regulatory debate and reforms that followed. This study also contributes to the governance literature that studies firm governance changes in response to management misconduct.

From a policy perspective, my findings provide important insights to regulators. Results are consistent with standard-setters' assertions that IAFs improve corporate governance by assisting audit committees in monitoring management. Thus, this study informs the debate relating to the recently released NASDAQ proposal that was subsequently withdrawn (SEC 2013b). The results are especially important given anecdotal evidence that IAF quality is low for some firms. For example, recent SEC enforcement actions implicate the IAF in aiding management in committing misconduct (e.g., SEC 2012a, 2012b, 2012c). Additionally, experimental and anecdotal evidence suggests that the internal audit profession has difficulty attracting the most qualified candidates (Ernst & Young 2007; Burton, Starliper, Summers, and Wood 2012). This evidence suggests that regulators, audit committees, and the internal audit profession have incentive to consider ways to increase IAF competence.

II. BACKGROUND, RELATED LITERATURE, AND HYPOTHESIS DEVELOPMENT

Governance-Related Reforms of the Early 2000s

In response to egregious accounting frauds at firms such as Enron and WorldCom, the U.S. Congress passed the Sarbanes-Oxley Act of 2002 (SOX), which included corporate governance reforms that affected audit committees and external auditors (Lucas 2004). SOX created the Public Company Accounting Oversight Board, which oversees external auditors and requires compliance with external auditing standards. SOX required the SEC to codify audit committee requirements such as member independence, disclosure of financial expertise, and the responsibility to select and oversee the external auditor. The SEC proposed these requirements and also requested comment on whether the appointment, compensation, retention, and oversight of the internal auditor should be part of the audit committee's responsibilities. However, the SEC abandoned the latter propositions, stating that the majority of commenters did not support them (SEC 2003b).[2] As a result, there are no SEC requirements that directly enforce IAF quality.

Also, in response to the high-profile corporate failures in the early 2000s, then-SEC Chairman Harvey Pitt asked the New York Stock Exchange and NASDAQ to review the corporate governance requirements for listed companies. Both exchanges implemented several changes to firm governance requirements, including strengthening audit committee independence requirements, requiring a majority of the board to be comprised of independent directors, and making the audit committee responsible for the appointment, compensation, retention, and oversight of the external auditor. In addition, the New York Stock Exchange added the requirement to have an IAF and that the audit committee periodically meets privately with management, internal auditors, and external auditors (SEC 2003c). However, these requirements do not stipulate minimum requirements for IAFs or require ongoing evaluations of the IAF (Protiviti 2007). Unlike the New York Stock Exchange, NASDAQ firms currently are not required to have an IAF. NASDAQ recently proposed such a requirement and stated that, among other things, it was designed to prevent fraud (SEC 2013a). However, it then withdrew the proposal to further consider issuer comments and is currently considering how to revise the proposal (NASDAQ 2013).

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[2] In fact, the majority of commenters did not comment at all on the IAF-related propositions. Examples of commenters that supported the proposal include the Institute of Internal Auditors, the American Institute of Certified Public Accountants, the National Association of Corporate Directors, and the California Public Employees' Retirement System. The New York Stock Exchange, Financial Executives International, PricewaterhouseCoopers, and Deloitte & Touche are among the commentators who did not support the proposal (SEC 2003a). Commenters who did not support the proposal usually cited as reasons either (1) an already over-burdened audit committee, or (2) an unintentional consequence of eroding management's sense of ownership of internal controls.

External auditors likely enforce some level of IAF quality on all firms subject to internal control audits. If an external auditor believes the IAF is of insufficient quality, then they can issue a material weakness in internal controls over financial reporting.[3] Also, to comply with regulatory standards, audit committees for U.S. banks are required to oversee and evaluate the performance of the IAF, and bank directors are encouraged to ensure their IAFs follow internal auditing standards (OCC 2003; Board of Governors of the Federal Reserve System 2013). While U.S. banking regulators have passed requirements that improve IAF quality, IAF quality for most U.S. firms is left largely up to audit committees and management.

The combined oversight of the IAF by the audit committee and management is often informal, sometimes contentious, and lacks clarity (Beasley, Carcello, Hermanson, and Neal 2009). After interviewing 42 audit committee members, Beasley et al. (2009, 103) conclude, "There is significant potential for internal audit's loyalties to be divided as a result of multiple reporting channels." Managers intending to commit misconduct have more opportunity to do so if they also control IAF quality.

Background on Internal Audit and IAF Quality as Defined by Standard-Setters

The Institute of Internal Auditors defines internal auditing as "an independent, objective assurance and consulting activity designed to add value and improve an organization's operations" (Reding et al. 2009, 1–2). Internal auditors help organizations achieve business objectives by improving the effectiveness of risk management, control, and governance processes, and are a valuable source of information for audit committees, external auditors, and managers (Gramling, Maletta, Schneider, and Church 2004; Protiviti 2007). Internal auditors serve as the "eyes and ears" of the audit committee and are able to deter management misconduct by effectively being able to detect management misconduct (AICPA 2005): "Fear of getting caught is always a strong deterrent" (AICPA 2008, 30). Internal auditors test internal controls throughout the organization and focus their efforts on areas with the highest risk. For example, at Lord Corporation, the IAF reviews all high-risk areas of the financial close process each year (Financial Executives Research Foundation [FERF] 2013). Internal auditors play a key role in monitoring compliance with laws and regulations such as the Foreign Corrupt Practices Act of 1977 (Birk 2010; Tabuena 2010). Also, the IAF itself serves as an important control. For example, the head of the internal audit department at IBM sits on the disclosure committee, helping to ensure the quality of disclosures (FERF 2013).

In addition to other responsibilities, internal auditors must consider the probability of fraud (IIA 2012). However, unlike external auditing standards, which focus only on fraud related to material misstatement of financial statements, fraud in internal auditing standards is broadly defined as: "Any illegal act characterized by deceit, concealment, or violation of trust ... Frauds are perpetrated by parties and organizations to obtain money, property, or services; to avoid payment or loss of services; or to secure personal or business advantage" (IIA 2012, 20). Thus, in addition to financial reporting fraud, IAFs consider management misconduct such as bribing foreign officials and issuing intentionally misleading disclosures. Therefore, a high-quality IAF should deter accounting- and nonaccounting-related management misconduct (Chadwick 2000; COSO 2012).

Standard-setters recognize that IAFs can deter management misconduct. Although the audit committee has the primary responsibility to monitor senior management, IAFs are a key resource in this task (AICPA 2005, 2008; COSO 2012). Additionally, external auditing standards state that an effective IAF can constrain management misconduct (AICPA 2007).

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[3] For fiscal years 2004 through 2011, Audit Analytics identified 85 of 33,036 auditor internal control opinions that disclosed a material weakness in internal control over financial reporting caused by an ineffective IAF. None of these material weaknesses involve sample firms.

Related Literature and Hypothesis Development

Two studies provide initial evidence that IAFs can deter management misconduct. First, Beasley et al. (2000) investigate a sample of 66 fraud firms subject to SEC Accounting and Auditing Enforcement Releases issued during 1987 through 1997, and report that fraud firms were less likely to have IAFs than a benchmark of non-fraud firms. Second, Schneider and Wilner (1990) report that IAF existence deters financial reporting irregularities under certain conditions.[4] These studies suggest that IAF existence deters management misconduct, but do not address whether IAF quality affects the likelihood of management misconduct incrementally to other monitors, such as audit committees and external auditors.

Prawitt et al. (2009) investigate the link between IAF and financial reporting quality. They find that IAF quality is unrelated to positive abnormal accruals, but negatively related to negative abnormal accruals (i.e., higher IAF quality is associated with lower magnitude negative abnormal accruals), and state that this is consistent with high-quality IAFs limiting big-bath behavior. Prawitt et al. (2009) also find that firms that just miss analysts' consensus forecasts have higher IAF quality compared to either firms that just meet analysts' consensus forecasts or all other firms. This provides some evidence of the effect of IAF quality on financial reporting quality, but does not directly address the effect on management misconduct.[5]

Studies that have recently investigated the effect of IAF quality on a commercially developed risk measure that serves as a proxy for fraudulently misstated financials present mixed results. While not a focus of either paper, Christ et al. (2012) find that IAF quality is unassociated with accounting risk, but Prawitt et al. (2012) find evidence that IAF quality is negatively associated with accounting risk.[6] Stated in the alternative form, the main hypothesis follows:

H1a: Internal audit function quality is negatively associated with the likelihood of management misconduct.

Two key components of IAF quality are competence and objectivity. Both external and internal auditing standards posit that a competent and objective IAF can enhance financial reporting quality (AICPA 1997; PCAOB 2010; IAASB 2012; IIA 2012).[7] Competence refers to the "knowledge, skills, and other competencies needed to perform ... responsibilities" (IIA 2012, 5), while objectivity refers to "an unbiased mental attitude that allows internal auditors to perform engagements in such a manner that they believe in their work product and that no quality compromises are made" (IIA 2012, 3). Two sub-hypotheses, stated in the alternative form, follow:

H1b: Internal audit function competence is negatively associated with the likelihood of management misconduct.

H1c: Internal audit function objectivity is negatively associated with the likelihood of management misconduct.

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[4] Specifically, in an experimental setting, Schneider and Wilner (1990) report that the existence of a competent IAF serves as a deterrent to financial reporting irregularities compared to a condition with no internal or external audits, when all of the following are present: material dollar amounts, irregularities that involve asset overstatement, unambiguous violation of Generally Accepted Accounting Principles, and managers with lower incentives for misstating income.

[5] For example, proxies for financial reporting quality, such as discretionary accruals, have little or no correlation with observable instances of intentional management misconduct (Price, Sharp, and Wood 2011).

[6] In these studies, accounting risk is defined as the risk of misleading or fraudulently misstated financials and is operationalized as a continuous, proprietary measure developed by Audit Integrity (Price et al. 2011).

[7] Internal auditing standards use the term "proficiency" instead of "competence" (see IIA 2012, Attribute Standard 1210). International Standards on Auditing (ISA) 610 (IAASB 2012) and the recently released Statement on Auditing Standards (SAS) No. 128 (AICPA 2014) recognize a third dimension of IAF quality: a "systematic and disciplined approach."

III. SAMPLE SELECTION

I obtained the initial sample from the Institute of Internal Auditors' proprietary Global Auditing Information Network database. This database includes survey responses from chief audit executives associated with the Institute of Internal Auditors and has been used in a small number of prior studies, including Prawitt et al. (2009) and Messier et al. (2011).[8] The Institute of Internal Auditors Research Foundation provided the data to me on conditions of anonymity and confidentiality. The data included firm names for years 2004 through 2009, but not for years 2000 through 2003. To identify firms in the earlier years, I first utilized the unique Global Auditing Information Network database firm identifier assigned to each firm in the database. That is, if a firm responded to the survey at least once from 2004 through 2009, then the firm name can be positively linked to the database firm identifier, which can then be used to identify that same firm's responses in years 2000 through 2003. Then, for the remaining firm-years from 2000 through 2003, I matched self-reported total assets, revenue, and employees to associated fields in Compustat, similar to prior research such as Prawitt et al. (2009) and Messier et al. (2011).

The sample covers 2000 through 2009. Following Prawitt et al. (2009), I remove firm-years with unreasonable values. These include the percentage of certified public accountants, certified internal auditors, or certified fraud examiners greater than 100 percent, average training hours greater than 160 per year, time spent providing direct assistance to the external auditors greater than 100 percent, and reported average internal audit experience greater than 30 years.[9] I include control variables related to board of directors, audit committee, and external auditor quality, as well as to other determinants of misconduct. Data for these control variables come from BoardEx, Compustat, Corporate Library, and Thomson Reuters. Table 1, Panel A presents the survey response frequency of the firms. Fifty-two percent of the firms (322 of 617) respond only one time during the sample time period, which would be consistent with those firms responding to the Global Auditing Information Network survey to benchmark their IAF against their industry peers and then not responding again for several years. My final sample includes 1,398 firm-years from 617 unique firms.

The management misconduct sample comes from four data sources. First, I gather SEC and Department of Justice enforcement actions that allege fraud or other intentional misconduct from the Federal Securities Regulation Database. Second, I gather settled securities class-action lawsuits from the Stanford Securities Class Action Clearinghouse. I exclude lawsuits that were dismissed because the plaintiffs in these cases were unable to convince the judge that there was fraudulent intent. Third and fourth, I obtain SEC and Department of Justice enforcement actions claiming violations of the foreign bribery provisions of the Foreign Corrupt Practices Act of 1977 from the SEC's and Department of Justice's websites.[10] For the first two sources, plaintiffs must establish intent under Section 17(a) of the Securities Act of 1933 or Section 10(b) or 13(b)(5) of the Securities Exchange Act of 1934. For the last two sources, the bribery provisions of the Foreign Corrupt Practices Act of 1977 require a "knowing" standard, which is a strict form of intent. Thus,

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[8] Firms that respond to the Institute of Internal Auditors' survey are provided with benchmarking data that they can use to compare their IAF to industry practices. Therefore, it is possible that survey firms have higher IAF quality, on average, than the rest of the population. This could bias against finding results consistent with the hypotheses because of low variation in IAF quality in the sample firms.

[9] Removing firm-years with unreasonable data eliminates 11 firm-years. Inferences remain the same if these firm-years are included within the analysis.

[10] More information on each of these data sources can be found at: http://www.fesreg.com/, http://securities.stanford.edu/index.html, http://www.sec.gov/spotlight/fcpa/fcpa-cases.shtml, and http://www.justice.gov/criminal/fraud/fcpa/cases/a.html, respectively. The data in the Federal Securities Regulation Database were hand-collected by Jerry Martin and initially used in Karpoff, Lee, and Martin (2008a, 2008b). While many bribery cases discuss bribery within the facts of the case, I include only those cases that allege violations of Section 30(a) of the Securities Exchange Act of 1934, which requires a "knowing standard," in the sample.

TABLE 1
Descriptives

Panel A: Response Frequency of Sample Firms

Number of Firm-Years for Each Firm	Number of Firms	Percent of Firms	Total Firm-Years
1	322	52%	322
2	114	18%	228
3	66	11%	198
4	45	7%	180
5	19	3%	95
6	18	3%	108
7	13	2%	91
8	10	2%	80
9	4	1%	36
10	6	1%	60
Total	617	100%	1,398

Total sample of firm-years = 1,398.
This panel provides a count and related percentage of how many times sample firms responded to the Global Auditing Information Network survey from 2000 through 2009.

Panel B: Misconduct Case Details

	Number of Unique Cases	Number Where the CEO is Named	Number Where the CFO is Named	Number Where the Company is Named	Number Where Other Managers are Named
SEC/DOJ Enforcement Action	14	8	8	14	13
Class-Action Lawsuit—accounting	31	31	31	31	28
Class-Action Lawsuit—not accounting	17	12	12	17	11
Bribery under the Foreign Corrupt Practices Act	6	0	0	6	5
Total	68	51	51	68	57

This panel includes details related to the misconduct cases. Misconduct cases are obtained from the Federal Securities Regulation Database, Stanford Securities Class Action Clearinghouse, SEC, and Department of Justice. There is substantial overlap among the unique cases noted above. The 68 unique cases correspond to 107 firm-years with misconduct. Out of these 107 firm-years, the CEO is named in 70, the CFO is named in 70, the company is named in 107, and other managers are named in 87.
The average three-day abnormal (market-adjusted) return surrounding the revelation day for the enforcement actions and lawsuits is −14.8 percent. This represents an average market capitalization loss of approximately $2.6 billion using the market capitalization of each firm on the first day of the three-day window. The average fine levied by the SEC and/or Department of Justice for the bribery cases is $44.7 million.

Panel C: Misconduct by Year, 2000–2009

Year	Misconduct	Total # of Observations	% with Misconduct
2000	13	75	17%
2001	16	101	16%
2002	16	101	16%

(continued on next page)

TABLE 1 (continued)

Year	Misconduct	Total # of Observations	% with Misconduct
2003	12	87	14%
2004	8	133	6%
2005	14	201	7%
2006	14	208	7%
2007	9	186	5%
2008	4	163	2%
2009	1	143	1%
Total	107	1,398	8%

This panel includes the number of misconduct years obtained from the Federal Securities Regulation Database, Stanford Securities Class Action Clearinghouse, SEC, and Department of Justice. There are 68 unique cases that correspond to 107 firm-years with misconduct. Variable definitions are provided in Appendix A.

Panel D: Firm-Years by Industry

Industry	Firm-Years	Firm-Years with Misconduct	% with Misconduct	Average *QUALITY*
Consumer Non-Durables	90	0	0%	2.88
Consumer Durables	34	1	3%	2.82
Manufacturing	132	1	1%	2.83
Oil, Gas, and Coal Extraction Products	48	2	4%	3.15
Chemicals and Allied Products	60	1	2%	2.88
Computer, Software, and Electronic Equipment	191	27	14%	2.86
Telephone and Television Transmission	60	9	15%	3.10
Utilities	243	8	3%	4.22
Wholesale, Retail, and Some Services	104	5	5%	3.02
Healthcare, Medical Equipment, and Drugs	63	12	19%	3.11
Finance	245	33	13%	3.53
Other	128	8	6%	3.19
Total	1,398	107		

This panel provides the number of firm-years and misconduct years in the sample by the Fama-French 12-industry classification. Misconduct includes (1) violation years from the Federal Securities Regulation Database where fraud or other intentional misconduct is alleged, (2) class period years for settled securities class-action lawsuits per the Stanford Securities Class Action Clearinghouse, and (3) violation years for SEC and Department of Justice Enforcement Actions claiming violation of the bribery provisions of the Foreign Corrupt Practices Act of 1977. Thus, misconduct is not the announcement of the misconduct, but rather the years when management is committing misconduct. *QUALITY* is a composite measure of IAF quality that ranges from 0 to 6, with 0 representing the lowest quality and 6 representing the highest quality.

Panel E: Sample Descriptive Statistics

	Mean	Std. Dev.	25th Pctl.	Median	75th Pctl.
Management Misconduct					
MISCONDUCT	0.077	0.266	0.000	0.000	0.000
IAF Quality					
QUALITY	3.286	1.292	2.000	3.000	4.000
COMPETENCE	0.000	1.000	−0.698	−0.087	0.605

(continued on next page)

TABLE 1 (continued)

	Mean	Std. Dev.	25th Pctl.	Median	75th Pctl.
OBJECTIVITY	0.000	1.000	−0.486	0.099	0.564
EXPERIENCE	6.358	3.676	3.852	6.000	8.275
CERTIFICATION	0.613	0.313	0.400	0.571	0.769
TRAINING	52.273	27.025	40.000	46.000	70.000
NOT_TRAINING_GROUND	0.667	0.472	0.000	1.000	1.000
INDEPENDENT	0.873	0.333	1.000	1.000	1.000
IAF_SIZE	0.000	1.393	−0.579	−0.303	0.213
Control Variables					
ASSISTS_AUDITOR	0.029	0.051	0.000	0.010	0.050
AC_ACCOUNTING_EXP	0.307	0.230	0.167	0.250	0.500
BRD_PCT_IND	0.667	0.105	0.615	0.667	0.706
CEOISCHAIR	0.657	0.465	0.000	1.000	1.000
CEOFOUNDER	0.046	0.211	0.000	0.000	0.000
INSTITUTIONAL_OWN	0.525	0.335	0.280	0.600	0.792
AUDITOR_SPECIALIST	0.338	0.473	0.000	0.000	1.000
LMARKETCAP	22.307	1.668	21.130	22.270	23.414
BM	0.545	0.617	0.293	0.471	0.681
LEVERAGE	0.262	0.173	0.132	0.256	0.364
PCTFORSALES	0.197	0.297	0.000	0.010	0.351
ISSUE	0.278	0.448	0.000	0.000	1.000

n = 1,398.
This panel provides descriptive statistics for the sample.
Variable definitions are provided in Appendix A.

(continued on next page)

the management misconduct sample represents instances where there is strong evidence of managerial intent to commit illegal acts.

Table 1, Panel B shows that the vast majority of cases (51 of 68) included in the sample specifically charge the CEO and/or CFO with intentional misconduct. The cases are associated with large losses of market capitalization and significant fines from regulators. For example, the average abnormal (market-adjusted) return over the three days surrounding the initial misconduct revelation is approximately −14.8 percent, which represents approximately $2.6 billion in market capitalization lost. The average regulator fine for bribery violations is approximately $44.7 million. Table 1, Panel C maps the 68 unique cases into 107 corresponding firm-years with misconduct. Out of these 107 firm-years, the CEO is named in 70, the CFO is named in 70, the company is named in 107, and other managers are named in 87.

Table 1, Panel D presents the breakdown of responses by industry, reporting misconduct rates and average IAF quality. Misconduct rates are the percentage of firm-years where management is alleged to have committed misconduct. The healthcare, medical equipment, and drugs industry has the highest rate of misconduct in the sample. The utility and finance industries have the highest average IAF quality, and the consumer durables and manufacturing industries have the lowest average IAF quality.

IV. RESEARCH DESIGN

Measures of IAF Quality

Auditing standards consider internal auditor professional experience, professional certifications, and training as evidence of competence (PCAOB 2010). I define *EXPERIENCE* as the

TABLE 1 (continued)

Panel F: Sample Descriptive Statistics by Misconduct

	MISCONDUCT = 1 (n = 107)			*MISCONDUCT* = 0 (n = 1,291)			Differences	
Variables	**Mean**	**Std. Dev.**	**Median**	**Mean**	**Std. Dev.**	**Median**	**Mean**	**Median**
IAF Quality								
QUALITY	2.589	1.189	2.000	3.344	1.284	3.000	−0.755***	−1.000***
COMPETENCE	−0.397	0.836	−0.441	0.033	1.006	−0.050	−0.430***	−0.391***
OBJECTIVITY	−0.223	0.966	−0.096	0.018	1.001	0.117	−0.241**	−0.213**
EXPERIENCE	5.455	3.279	5.000	6.432	3.698	6.000	−0.977***	−1.000***
CERTIFICATION	0.510	0.220	0.485	0.622	0.318	0.583	−0.112***	−0.098***
TRAINING	52.206	28.047	46.000	52.279	26.950	46.000	−0.073	0.000
NOT_TRAINING_GROUND	0.551	0.500	1.000	0.676	0.468	1.000	−0.125***	0.000***
INDEPENDENT	0.813	0.392	1.000	0.878	0.328	1.000	−0.065*	0.000*
IAF_SIZE	−0.406	0.586	−0.590	0.034	1.435	−0.283	−0.439***	−0.307***
Control Variables								
ASSISTS_AUDITOR	0.020	0.033	0.000	0.030	0.052	0.010	−0.010***	−0.010
AC_ACCOUNTING_EXP	0.225	0.209	0.250	0.313	0.230	0.286	−0.088***	−0.036***
BRD_PCT_IND	0.653	0.093	0.650	0.668	0.106	0.667	−0.016*	−0.017
CEOISCHAIR	0.774	0.397	1.000	0.647	0.469	1.000	0.127***	0.000***
CEOFOUNDER	0.103	0.305	0.000	0.042	0.200	0.000	0.061***	0.000***
INSTITUTIONAL_OWN	0.368	0.338	0.419	0.538	0.331	0.614	−0.170***	−0.195***
AUDITOR_SPECIALIST	0.346	0.478	0.000	0.337	0.473	0.000	0.009	0.000
LMARKETCAP	23.415	1.734	23.689	22.215	1.630	22.203	1.200***	1.486***
BM	0.595	1.213	0.397	0.541	0.540	0.474	0.054	−0.077**
LEVERAGE	0.271	0.221	0.237	0.261	0.168	0.258	0.010	−0.021
PCTFORSALES	0.282	0.573	0.107	0.190	0.260	0.005	0.092*	0.102**
ISSUE	0.318	0.468	0.000	0.275	0.447	0.000	0.043	0.000

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*, **, *** Represent two-tailed statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

The *MISCONDUCT* sample includes (1) violation years from the Federal Securities Regulation Database where fraud or other intentional misconduct is alleged, (2) class period years for settled securities class-action lawsuits per the Stanford Securities Class Action Clearinghouse, and (3) violation years for SEC and Department of Justice Enforcement Actions claiming violations of the bribery provisions of the Foreign Corrupt Practices Act of 1977. I construct IAF quality measures using the Institute of Internal Auditors' Global Auditing Information Network database from 2000 through 2009.

Variable definitions are provided in Appendix A.

average number of years of internal auditing experience in the IAF; *CERTIFICATION* as the percentage of internal auditors who have the Certified Internal Auditor (CIA), Certified Public Accountant (CPA), or Certified Fraud Examiner (CFE) certifications; and *TRAINING* as the average number of hours of training internal auditors completed during the year.[11] Then, following prior research, I create dichotomous variables for each by assigning a value of 1 to the variable if it is above the median of the sample, and 0 otherwise. These variables are *EXPERIENCE_IND*, *CERTIFICATION_IND*, and *TRAINING_IND*, respectively.

Auditing standards state that auditor independence affects auditor objectivity. Thus, I set *INDEPENDENT* to 1 if the IAF functionally reports to the audit committee, and 0 otherwise. Additionally, IAFs that are used as a management training ground are perceived as less objective by external auditors (Messier et al. 2011). Thus, I set *NOT_TRAINING_GROUND* to 1 if the IAF is not used as a management training ground, and 0 otherwise.[12]

To capture a firm's overall IAF investment, I create an IAF size measure similar to Prawitt et al. (2009). I first calculate the dollar amount spent on internal auditing per firm in year *t* divided by the firm's total sales at the end of year *t*. *IAF_SIZE* is calculated by subtracting the industry average of this ratio from the firm-specific ratio and then dividing this amount by the industry-average of the ratio of dollar amount spent on internal auditing to sales.[13] I then create a dichotomous variable, *IAF_SIZE_IND*, which is equal to 1 if *IAF_SIZE* is positive, and 0 otherwise. I expect better-funded IAFs to be more effective at monitoring management because they have more resources at their disposal.

Finally, I create a comprehensive IAF quality measure similar to Prawitt et al. (2009). *QUALITY* is the sum of *EXPERIENCE_IND*, *CERTIFICATION_IND*, *TRAINING_IND*, *NOT_TRAINING_GROUND*, *INDEPENDENT*, and *IAF_SIZE_IND*. *QUALITY* ranges from 0 to 6, with larger numbers indicating better overall IAF quality.

I also create measures of IAF competence and objectivity by using a principal components factor analysis to extract common variation among the raw measures of quality, *EXPERIENCE*, *CERTIFICATION*, *TRAINING*, *NOT_TRAINING_GROUND*, *INDEPENDENT*, and *IAF_SIZE*. I first standardize each measure to have a mean of zero and a standard deviation of 1. The factor analysis identified two factors with eigenvalues greater than 1, which collectively explain approximately 44 percent of the underlying variance. The first factor is positively associated with *EXPERIENCE*, *CERTIFICATION*, *TRAINING*, *NOT_TRAINING_GROUND*, and *IAF_SIZE*, and negatively associated with *INDEPENDENT*. The strongest factor loadings are for *EXPERIENCE* and *CERTIFICATION*, consistent with how auditing standard-setters define competence, so I label this first factor *COMPETENCE*. The second factor is negatively associated with *EXPERIENCE*, *CERTIFICATION*, and *TRAINING*, and positively associated with *NOT_TRAINING_GROUND*,

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[11] Distinct from prior research that utilizes the Global Auditing Information Network survey, I include the CFE within *CERTIFICATION* because it is a certification that is directly related to fraud prevention and detection.

[12] For years 2000 through 2005, the Global Auditing Information Network survey asks participants "Are staff auditors encouraged to make auditing their career?" with a possible response being "Rotational/prepare for other areas of the company." I set *NOT_TRAINING_GROUND* to 0 for firm-years when this response is given, and to 1 when other responses are selected. The Global Auditing Information Network survey also captures the percentage of professional staff turnover that was placed inside the company. For years 2000 through 2005, this percentage was 15.6 percent for firm-years where the IAF was used as a management training ground. Therefore, for years 2006 through 2009, I set *NOT_TRAINING_GROUND* to 0 for firm-years where the percentage of professional staff turnover that was placed inside the company was greater than 15.6 percent. Inferences remain the same if I exclude years 2006 through 2009 from the sample.

[13] The formula is: [(firm IAF budget/firm sales) – (industry average of firm IAF budget/firm sales)]/(industry average of firm IAF budget/firm sales). I use the Fama-French 12-industry classification. The number of firm-years by industry ranges from 34 to 245, with an average of 117 and a median of 97.

INDEPENDENT, and *IAF_SIZE*. The strongest factor loading is for *INDEPENDENT*, so I label this second factor *OBJECTIVITY*.

Management Misconduct Model

The management misconduct model controls for incentives to engage in misconduct, corporate governance variables related to monitoring, and other determinants of misconduct. Specifically, I control for growth prospects, leverage, audit committee accounting expertise, director independence, external auditor industry expertise, chief executive officer duality, founders who are also the chief executive officer, institutional holdings, debt and equity offerings, and size (Dechow, Sloan, and Sweeney 1996; Klein 2002; Dechow et al. 2011; Donelson, McInnis, and Mergenthaler 2012; Badolato, Donelson, and Ege 2014). I also control for the extent to which the IAF assists the external auditor and the percentage of sales attributable to foreign segments. With variable definitions in Appendix A, the model is:

$$\begin{aligned} Prob(MISCONDUCT_t = 1) = F(&\beta_0 + \beta_1 QUALITY_t + \beta_2 ASSISTS\_AUDITOR_t \\ &+\beta_3 AC\_ACCOUNTING\_EXP_t + \beta_4 BRD\_PCT\_IND_t \\ &+\beta_5 CEOISCHAIR_t + \beta_6 CEOFOUNDER_t \\ &+\beta_7 INSTITUTIONAL\_OWN_t + \beta_8 AUDITOR\_SPECIALIST_t \\ &+\beta_9 LMARKETCAP_t + \beta_{10} BM_t + \beta_{11} LEVERAGE_t \\ &+\beta_{12} PCTFORSALES_t + \beta_{13} ISSUE_t + Year\ Fixed\ Effects \\ &+Industry\ Fixed\ Effects). \end{aligned} \tag{1}$$

The dependent variable, *MISCONDUCT*, is an indicator variable equal to 1 for (1) violation years from the Federal Securities Regulation Database where fraud or other intentional misconduct is alleged, (2) class period years for settled securities class-action lawsuits per the Stanford Securities Class Action Clearinghouse, and (3) violation years for SEC and Department of Justice Enforcement Actions claiming violation of the bribery provisions of the Foreign Corrupt Practices Act of 1977.[14] Thus, each instance where *MISCONDUCT* is equal to 1 represents a firm-year where management allegedly intentionally committed illegal acts.

The coefficient of interest is β_1, which measures the effect of IAF quality. A negative and significant coefficient would be consistent with H1a. To test H1b and H1c, I replace *QUALITY* with *COMPETENCE* and *OBJECTIVITY*. A negative and significant coefficient on *COMPETENCE* and *OBJECTIVITY* would be consistent with H1b and H1c, respectively.

IAFs are often used by external auditors to perform financial statement audit tasks. External auditors predominantly use IAFs for assistance in low-risk areas, which are unlikely to have a high risk of management misconduct (AICPA 1997; PCAOB 2010). I define *ASSISTS_AUDITOR* as the percentage of time the IAF spends assisting the external auditor in year *t*. I do not make a directional prediction for this variable. On the one hand, more direct assistance lowers the amount of time IAFs can spend on higher-risk areas, which would lead to a positive prediction. However, external auditors cannot rely on IAFs unless they are of high quality, which suggests that *ASSISTS_AUDITOR* could be negatively related to the likelihood of management misconduct.

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[14] For example, assume firm-years 2005 through 2009 for firm *i* are included in the Global Auditing Information Network database. Also, assume an SEC enforcement action was announced in 2009 and alleged that management of firm *i* falsified accounting records from January 1, 2007 through December 31, 2008. Then, *MISCONDUCT* would equal 1 for firm-years 2007 and 2008, and 0 for firm-years 2005, 2006, and 2009. For firms that are in the Global Auditing Information Network database for only one firm-year, *MISCONDUCT* for that firm-year is equal to 1 if there was ongoing misconduct during that year, and 0 otherwise.

I control for governance-related characteristics that affect governance quality and could affect the likelihood of management misconduct. I control for the accounting expertise of the audit committee, as prior literature suggests audit committee accounting expertise improves audit committee effectiveness. For example, accounting expertise on the audit committee is associated with better financial reporting quality, as measured by better accrual quality and more conservatism (Krishnan and Visvanathan 2008; Dhaliwal, Naiker, and Navissi 2010). *AC_ACCOUNTING_EXP* equals the percentage of audit committee members with accounting expertise in year *t*. Data for this variable come from BoardEx. Accounting expertise is defined as work experience as a public auditor at one of the 25 audit firms listed in Compustat, as a CPA or Chartered Accountant, or in an accounting-specific position, such as Chief Financial Officer, Treasurer, Controller, or Head of Accounting. I expect a negative relation between the percentage of audit committee accounting experts and the likelihood of management misconduct. I control for board independence because boards must have a majority of independent directors and independence is a key tenet of strong monitoring (Beasley 1996; Klein 2002; Larcker, Richardson, and Tuna 2007; Donelson et al. 2012). I expect a negative relation between the percentage of independent board members and the likelihood of management misconduct. *BRD_PCT_IND* is equal to the percentage of directors that is independent in year *t*.

I control for chief executive officer duality and chief executive officers who are also the founder of the company. *CEOISCHAIR* is equal to 1 if the chief executive officer is also chair of the board of directors in year *t*, and 0 otherwise. *CEOFOUNDER* is equal to 1 if the chief executive officer in year *t* is also the company founder, and 0 otherwise. I expect a positive relation between each of these variables and the likelihood of management misconduct, because they represent instances where the chief executive officer has high levels of influence over company operations and the board of directors (Fama and Jensen 1983; Yermack 1996; Dechow et al. 1996).

I also control for institutional ownership and external auditor quality. Large block shareholders with voting rights, such as institutional investors, have strong incentives to monitor management and, thus, could serve to decrease the likelihood of management misconduct (Shleifer and Vishny 1986; Beasley 1996). *INSTITUTIONAL_OWN* is the percentage of shares outstanding held by institutional investors at the end of year *t*. I expect *INSTITUTIONAL_OWN* to be negatively related to the likelihood of management misconduct. I also control for external auditor quality by identifying whether firms are audited by an industry-expert external auditor. *AUDITOR_SPECIALIST* is equal to 1 if the external auditor has the highest industry market share for year *t*, and 0 otherwise (Palmrose 1986).[15] I expect *AUDITOR_SPECIALIST* to be negatively related to the likelihood of management misconduct because industry-expert auditors should provide higher-quality external audits.

I also control for size, growth prospects, leverage, percentage of foreign sales, and debt and equity offerings. *LMARKETCAP* is the natural log of market capitalization at the end of year *t*. *BM* is the book value of common equity divided by the market value of common equity at the end of year *t*. *LEVERAGE* is total debt scaled by total assets at the end of year *t*. *PCTFORSALES* is the percentage of total sales attributable to foreign segments as reported in the Compustat Segments database at the end of year *t*. *ISSUE* indicates whether a firm issued significant new debt or equity. It is equal to 1 when the sum of issued securities during year *t* is greater than 10 percent of total assets at the end of year *t*. When examining relations between these variables and management misconduct, prior research finds a positive relation with firm size, a negative relation with the book-to-market ratio, a positive (although not always significant) relation with leverage, and a positive relation with security issuance (e.g., Beneish 1999; Dechow et al. 2011). Thus, I expect a positive sign on *LMARKETCAP*, a negative sign on *BM*, a positive sign on *LEVERAGE*, and a positive sign on *ISSUE*. I expect a negative sign on *PCTFORSALES* because firms with large foreign operations are more likely to violate the bribery provisions of the Foreign Corrupt Practices Act of 1977 than firms that only operate domestically.

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[15] I do not use an indicator for the presence of a Big 4 auditor because only 67 sample firm-years have a non-Big 4 auditor. Inferences remain the same if I include an indicator for a Big 4 auditor instead of controlling for industry expertise. Inferences also remain the same if I define industry-expert auditors based upon expertise at the city level or based upon joint city- and national-level expertise (Francis, Reichelt, and Wang 2005).

Response by Misconduct Firms

If participants in the governance process believe IAF quality serves as a deterrent of management misconduct, then misconduct firms should respond to revealed misconduct by increasing IAF quality.[16] For example, as noted in Section I, Amgen recently agreed to improve its IAF quality after illegally marketing two drugs. Evidence of an increase in IAF quality for misconduct firms in the years subsequent to management misconduct would provide some support for a causal interpretation of the results in Equation (1). That is, IAF quality would be negatively related to the likelihood of management misconduct because of firms' beliefs in the deterrent effects of IAF quality.

I examine whether IAF quality changes for misconduct firms following misconduct using a difference-in-differences design. I match firm-years from misconduct firms to firm-years from non-misconduct firms based upon year, Fama-French 12-industry classification, and closest market capitalization. The final sample includes 179 firm-years from misconduct firms, of which 105 are from misconduct years and 74 are from post-misconduct years, and a matched set of 179 firm-years from firms that never had misconduct during the sample period. I estimate the following OLS model:

$$\begin{aligned}\mathit{QUALITY}_t = {} & \beta_0 + \beta_1 \mathit{MISCONDUCT\_FIRM}_t + \beta_2 \mathit{POST\_MISCONDUCT}_t \\ & + \beta_3 \mathit{MISCONDUCT\_FIRM}_t {*} \mathit{POST\_MISCONDUCT}_t + \beta_4 \mathit{ASSISTS\_AUDITOR}_t \\ & + \beta_5 \mathit{AC\_ACCOUNTING\_EXP}_t + \beta_6 \mathit{BRD\_PCT\_IND}_t + \beta_7 \mathit{CEOISCHAIR}_t \\ & + \beta_8 \mathit{CEOFOUNDER}_t + \beta_9 \mathit{INSTITUTIONAL\_OWN}_t \\ & + \beta_{10} \mathit{AUDITOR\_SPECIALIST}_t + \beta_{11} \mathit{LMARKETCAP}_t + \beta_{12} \mathit{BM}_t \\ & + \beta_{13} \mathit{LEVERAGE}_t + \beta_{14} \mathit{PCTFORSALES}_t + \beta_{15} \mathit{ISSUE}_t + \mathit{Year\ Fixed\ Effects} \\ & + \mathit{Industry\ Fixed\ Effects} + \varepsilon. \end{aligned} \tag{2}$$

MISCONDUCT_FIRM is an indicator variable that is set to 1 for observations from a firm that had misconduct during the sample period, and 0 otherwise. For example, if firm *i* engaged in misconduct during 2003 and 2004 and was in the sample from 2000 through 2009, then *MISCONDUCT_FIRM* would equal 1 for firm-years 2003 through 2009, and firm-years 2000 through 2002 would be excluded from the analysis as they are prior to the alleged misconduct. *POST_MISCONDUCT* is an indicator variable that is set to 1 for firm-years from misconduct firms that are subsequent to the violation years (i.e., where *MISCONDUCT* = 0 for year *t*, but *MISCONDUCT* = 1 for year *t–x*) and for matched firm-years from the control sample. There are two coefficients of interest. First, β_1 represents the difference in IAF quality during the misconduct years between misconduct firms and the control firms. A negative and significant coefficient on β_1 would suggest that IAF quality for misconduct firms is lower during the misconduct years as compared to the control firms. Second, β_3 represents the difference between misconduct firms and non-misconduct firms in the change of IAF quality from misconduct years to post-misconduct years; i.e., it is the difference-in-differences. A positive and significant coefficient on the interaction of *MISCONDUCT_FIRM* and *POST_MISCONDUCT* would be consistent with the proposition that those in charge of governance at misconduct firms responded to revealed misconduct by improving IAF quality, and that this response was not due to confounding factors.

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[16] Farber (2005) finds that firms that fraudulently manipulated their financial statements improved certain corporate governance characteristics, other than IAF characteristics, compared to a control sample within three years after fraud detection.

The remaining variables are defined the same as in Equation (1). To provide insights into whether misconduct firms increase IAF competence and/or objectivity in response to management misconduct, I replace the dependent variable, *QUALITY*, with *COMPETENCE* and *OBJECTIVITY* in subsequent specifications of Equation (2).

V. DESCRIPTIVE STATISTICS AND RESULTS

Descriptive Statistics

Table 1, Panel E presents descriptive statistics for the variables in Equation (1), as well as for the underlying IAF quality measures. The average years of internal auditing experience for the IAFs within my sample is 6.36 years. The average percentage of internal auditors who are professionally certified is 61 percent. This percentage represents 4, 37, and 20 percent of internal auditors who have the CFE, CPA, and CIA certifications, respectively. Internal auditors spend an average of 52 hours in training each year. For two-thirds of firm-years in my sample, the IAF is not used as a management training ground. The IAF functionally reports to the audit committee in approximately 87 percent of the firm-years within my sample. This percentage increases from 2000 through 2009, with a significant increase after the passage of SOX. Specifically, *INDEPENDENT* is 51 percent in 2000, 58 percent in 2001, 80 percent in 2002, 86 percent in 2003, and above 90 percent for 2004 through 2009.

Table 1, Panel F provides descriptive statistics for both the misconduct and non-misconduct years. The composite IAF quality measure is statistically higher for the sample without management misconduct. The IAF competence, objectivity, and underlying IAF quality measures are also statistically higher for the sample without management misconduct with the exception of *TRAINING*. Overall, these univariate results are consistent with the proposition that IAF quality, competence, and objectivity deter management misconduct.

Table 1, Panel F also shows that firm-years without management misconduct have a higher percentage of IAF hours used as direct assistance for external auditors. Consistent with predictions, firm-years with management misconduct have a lower percentage of audit committee members with accounting expertise, are more likely to have a chief executive officer who also chairs the board of directors or who is also the founder of the firm, have lower institutional ownership, have a larger market capitalization, and have a larger percentage of foreign sales. Also, the median book-to-market ratio is smaller for firm-years with management misconduct compared to firm-years without management misconduct, which is consistent with expectations.

Table 2 presents Pearson correlations between variables of interest. *QUALITY, COMPETENCE,* and *OBJECTIVITY* are negatively correlated with management misconduct, consistent with the proposition that overall IAF quality, as well as its underlying components, deters management misconduct. The percentage of audit committee accounting experts and independent board members are positively correlated with the composite measure of IAF quality, and chief executive officer duality is negatively associated with the composite measure of IAF quality. These correlations are consistent with better-governed firms having higher-quality IAFs.

Multivariate Levels Analysis

Table 3 provides results of testing H1a, H1b, and H1c using Equation (1). Consistent with H1a, the coefficient on *QUALITY* is negative and significant in column (1), suggesting IAF quality is

TABLE 2

Pearson Correlations

Panel A: Correlation Variables *ALL* to *ASSISTS_AUDITOR*

	Variables	1	2	3	4	5	6	7	8	9	10	11
1	*ALL*											
2	*QUALITY*	**−0.155**										
3	*COMPETENCE*	**−0.114**	**0.711**									
4	*OBJECTIVITY*	**−0.064**	**0.151**	0.000								
5	*EXPERIENCE*	**−0.071**	**0.521**	**0.808**	0.004							
6	*CERTIFICATION*	**−0.095**	**0.428**	**0.652**	**−0.160**	**0.288**						
7	*TRAINING*	−0.001	**0.263**	**0.046**	**−0.565**	−0.005	−0.002					
8	*NOT_TRAINING_GROUND*	**−0.070**	**0.550**	**0.586**	**0.244**	**0.289**	**0.078**	−0.014				
9	*INDEPENDENT*	**−0.051**	**0.309**	**−0.066**	**0.636**	−0.016	−0.029	**−0.046**	**0.076**			
10	*IAF_SIZE*	**−0.084**	**0.204**	**0.190**	**0.589**	**0.090**	**0.049**	**−0.088**	**0.074**	**0.075**		
11	*ASSISTS_AUDITOR*	**−0.053**	**0.060**	0.035	**0.044**	0.042	−0.021	0.012	**0.063**	**0.057**	0.009	
12	*AC_ACCOUNTING_EXP*	**−0.102**	**0.059**	−0.002	**0.125**	−0.043	0.006	**−0.059**	**0.061**	**0.093**	**0.064**	−0.005
13	*BRD_PCT_IND*	−0.040	**0.064**	0.025	**0.049**	**0.045**	**−0.073**	**0.054**	**0.074**	**0.049**	**0.049**	−0.020
14	*CEOISCHAIR*	**0.073**	**−0.070**	**−0.068**	**−0.153**	−0.013	**−0.074**	**0.072**	**−0.071**	**−0.105**	**−0.119**	**−0.077**
15	*CEOFOUNDER*	**0.077**	0.014	−0.020	**0.068**	−0.043	−0.004	**−0.067**	0.034	0.044	0.006	−0.039
16	*INSTITUTIONAL_OWN*	**−0.135**	0.035	0.039	**0.083**	0.008	−0.007	0.028	**0.099**	**0.098**	**0.046**	**0.114**
17	*AUDITOR_SPECIALIST*	0.005	−0.009	**−0.050**	**−0.074**	−0.043	−0.026	**0.082**	**−0.057**	**−0.045**	−0.009	**−0.084**
18	*LMARKETCAP*	**0.191**	**−0.251**	**−0.263**	**−0.315**	**−0.181**	**−0.179**	**0.230**	**−0.182**	**−0.091**	**−0.310**	**−0.181**
19	*BM*	0.023	**0.095**	**0.145**	**0.089**	**0.130**	**0.065**	−0.031	**0.105**	**0.051**	**0.084**	**0.097**
20	*LEVERAGE*	0.016	**0.058**	**0.060**	**−0.096**	**0.051**	0.029	**0.083**	0.027	**−0.069**	−0.029	−0.037
21	*PCTFORSALES*	**0.082**	**−0.212**	**−0.198**	−0.022	**−0.198**	**−0.082**	−0.023	**−0.136**	**−0.050**	−0.005	**−0.071**
22	*ISSUE*	0.025	−0.002	0.017	**0.045**	−0.040	**0.073**	**−0.078**	0.012	−0.012	0.043	0.010

(continued on next page)

TABLE 2 (continued)

Panel B: Correlation Variables *BRD_PCT_IND* to *ISSUE*, continued from Panel A

	Variables	12	13	14	15	16	17	18	19	20	21
12	*AC_ACCOUNTING_EXP*										
13	*BRD_PCT_IND*	**0.065**									
14	*CEOISCHAIR*	**−0.161**	**−0.066**								
15	*CEOFOUNDER*	**0.077**	**−0.072**	**0.079**							
16	*INSTITUTIONAL_OWN*	0.035	−0.042	−0.017	**−0.057**						
17	*AUDITOR_SPECIALIST*	**−0.061**	0.017	**0.068**	0.000	0.022					
18	*LMARKETCAP*	**−0.108**	−0.004	**0.149**	−0.013	−0.020	**0.191**				
19	*BM*	−0.004	−0.014	−0.006	**−0.044**	−0.022	−0.001	**−0.283**			
20	*LEVERAGE*	0.041	**0.107**	0.040	**−0.080**	**−0.059**	0.042	−0.007	**−0.070**		
21	*PCTFORSALES*	**0.067**	−0.008	−0.023	**0.051**	−0.040	**0.081**	**0.186**	**0.140**	**−0.170**	
22	*ISSUE*	**0.079**	−0.031	**−0.065**	**0.060**	**−0.053**	−0.038	**−0.154**	0.027	**0.372**	**−0.049**

Bold coefficients are significant at the 10 percent level.
Variable definitions are provided in Appendix A.

TABLE 3

Effect of IAF Quality on Management Misconduct
Prob($MISCONDUCT = 1$) = F($\beta_0 + \beta_1 QUALITY$ + Controls)

Independent Variables	**Pred. Sign**	**(1) Coefficient**	**(2) Ave. Marginal Eff.**	**(3) Coefficient**	**(4) Ave. Marginal Eff.**
QUALITY	–	−0.3213***	−0.0175***		
		(0.124)	(0.007)		
COMPETENCE	–			−0.2865**	−0.0158**
				(0.165)	(0.009)
OBJECTIVITY	–			0.0453	0.0025
				(0.148)	(0.008)
ASSISTS_AUDITOR	?	−1.8808	−0.1024	−1.7219	−0.0947
		(3.170)	(0.172)	(3.297)	(0.181)
AC_ACCOUNTING_EXP	–	−1.4528**	−0.0791**	−1.4927**	−0.0821**
		(0.800)	(0.043)	(0.817)	(0.044)
BRD_PCT_IND	–	−1.8688	−0.1018	−1.8515	−0.1018
		(1.962)	(0.108)	(1.996)	(0.111)
CEOISCHAIR	+	0.3171	0.0173	0.3870	0.0213
		(0.347)	(0.019)	(0.347)	(0.019)
CEOFOUNDER	+	1.0465**	0.0733	1.0298**	0.0724*
		(0.606)	(0.051)	(0.583)	(0.049)
INSTITUTIONAL_OWN	–	−1.3574***	−0.0739***	−1.3087***	−0.0720***
		(0.540)	(0.029)	(0.539)	(0.029)
AUDITOR_SPECIALIST	–	−0.1100	−0.0059	−0.1317	−0.0072
		(0.358)	(0.019)	(0.359)	(0.019)
LMARKETCAP	+	0.3173***	0.0173***	0.3403***	0.0187***
		(0.129)	(0.007)	(0.136)	(0.007)
BM	–	0.4715**	0.0257**	0.5326**	0.0293**
		(0.216)	(0.011)	(0.219)	(0.012)
LEVERAGE	+	1.3752*	0.0749*	1.6203**	0.0891**
		(0.936)	(0.049)	(0.938)	(0.049)
PCTFORSALES	+	1.2710*	0.0692	1.3074	0.0719
		(0.988)	(0.055)	(1.038)	(0.058)
ISSUE	+	0.0672	0.0037	0.0746	0.0041
		(0.349)	(0.019)	(0.352)	(0.019)
Year Fixed Effects?		Yes	Yes	Yes	Yes
Industry Fixed Effects?		Yes	Yes	Yes	Yes
Obs. where *MISCONDUCT* = 1		107	107	107	107
Total Obs.		1,398	1,398	1,398	1,398
Pseudo R^2		0.36	NA	0.35	NA

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*, **, *** Denote statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a one-tailed test where the coefficient sign is consistent with the predicted direction.

This table reports the results of a logistic regression examining the effect of IAF quality on the likelihood of management misconduct based on the following model: Prob($MISCONDUCT = 1$) = $F(\beta_0 + \beta_1 QUALITY$ + Controls).

The misconduct sample includes (1) violation years from the Federal Securities Regulation Database where fraud or other intentional misconduct is alleged, (2) class period years for settled securities class-action lawsuits per the Stanford

(continued on next page)

TABLE 3 (continued)

Securities Class Action Clearinghouse, and (3) violation years for SEC and Department of Justice Enforcement Actions claiming violations of the bribery provisions of the Foreign Corrupt Practices Act of 1977. In column (3), the composite IAF quality measure is replaced with *COMPETENCE* and *OBJECTIVITY*. Marginal effects presented in columns (2) and (4) are computed using the average of discrete or partial changes over all observations (Bartus 2005).
Standard errors clustered by firm are in parentheses. Pseudo R^2 is the max re-scaled R^2 from SAS (also known as Cragg and Uhler's pseudo R^2).
Variable definitions are provided in Appendix A.

negatively related to the likelihood of management misconduct. The average marginal effects are presented in column (2) and suggest that the coefficient on IAF quality is economically significant. A firm with IAF quality that is one standard deviation above the mean is approximately 2.3 percentage points less likely to have management misconduct as compared to the firm with average IAF quality.[17] This is approximately 29.5 percent of the unconditional probability of management misconduct of 7.7 percent.

Consistent with H1b, the coefficient on *COMPETENCE* in column (3) of Table 3 is negative and significant. This suggests IAF competence deters management misconduct, and the average marginal effect presented in column (4) suggests this effect is economically significant. A firm with IAF competence that is one standard deviation above the mean is approximately 1.6 percentage points less likely to have management misconduct as compared to the firm with average IAF competence. This is approximately 20.6 percent of the unconditional probability of management misconduct of 7.7 percent. Surprisingly, the coefficient on *OBJECTIVITY* in column (3) is unrelated to the likelihood of management misconduct. Thus, the evidence does not support H1c.[18,19]

I include year fixed effects in Equation (1) because shifting governance requirements throughout the sample period could affect the results of Equation (1). To further allay these concerns, I reestimate Equation (1) separately for years before (2000–2001) and after (2002–2009) the passage of SOX and for years before (2000–2003) and after (2004–2009) implementation of required internal control audits as part of SOX Section 404(b). Untabulated results show that inferences are similar for each period.[20] Additionally, as the IAF is part of the internal control structure of the firm, a firm that is committed to strong internal controls should have both a strong IAF and also strong internal controls over financial reporting. Thus, it is possible that a strong IAF is just one of many types of internal controls that would affect the likelihood of management misconduct. To help allay concerns that internal control quality is a correlated omitted variable, I reestimate Equation (1) and include an indicator variable that is set to 1 for firm-years with material weaknesses in internal control over financial reporting, as disclosed in SOX Section 302 and 404 disclosures. These internal control disclosures are available starting in 2002 and, therefore, I exclude firm-years 2000 and 2001 from the analysis. The coefficients on IAF quality and competence remain negative and significant after controlling for internal control quality (untabulated).[21]

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[17] For the calculation of the marginal effect of *QUALITY*, the change in likelihood of management misconduct is calculated by multiplying the coefficient on *QUALITY* in column (2) by the standard deviation of *QUALITY*. A similar calculation is done to determine the marginal effect of *COMPETENCE* below by multiplying the coefficient on *COMPETENCE* in column (4) by the standard deviation of *COMPETENCE*.

[18] In untabulated results, I reestimate Equation (1) and include either *COMPETENCE* or *OBJECTIVITY*, but not both at the same time. In these specifications, *COMPETENCE* is once again significant, and *OBJECTIVITY* remains insignificant. This suggests that it is control variables, not *COMPETENCE*, that are removing the significant effect of *OBJECTIVITY* found in univariate results. Specifically, including year fixed effects and/or *LMARKETCAP* results in an insignificant coefficient on *OBJECTIVITY*.

[19] To see which particular aspects of IAF quality are related to management misconduct, I reestimate Equation (1) and replace *QUALITY* with *EXPERIENCE*, *CERTIFICATION*, *TRAINING*, *NOT_TRAINING_GROUND*, *INDEPENDENT*, and *IAF_SIZE*. In untabulated results, only *CERTIFICATION* is statistically significant (one-tailed p-value less than 5 percent). This suggests that IAFs with higher levels of certification are associated with lower levels of management misconduct.

[20] The coefficient on *QUALITY* remains negative and significant in each time period. The coefficient on *COMPETENCE* remains negative and significant in each time period except for 2000 through 2003.

Response by Misconduct Firms

Table 4 provides results for Equation (2). The first column presents results when the dependent variable is *QUALITY*. Consistent with expectations, the coefficient on *MISCONDUCT_FIRM* is negative and significant, indicating that IAF quality is lower for misconduct firms during misconduct years compared to control firms. Additionally, the coefficient on the interaction of *MISCONDUCT_FIRM* and *POST_MISCONDUCT* is positive and significant. This is consistent with misconduct firms improving IAF quality in response to revealed management misconduct.

Columns (2) and (3) of Table 4 provide results when the dependent variables are *COMPETENCE* and *OBJECTIVITY*, respectively. The results for IAF competence are consistent with the results for IAF quality. That is, the coefficient on *MISCONDUCT_FIRM* is negative and significant, and the coefficient on the interaction of *MISCONDUCT_FIRM* and *POST_MISCONDUCT* is positive and significant. These results suggest that misconduct firms had lower IAF competence during misconduct years as compared to control firms, but then took steps to incrementally increase IAF competence in the post-misconduct years.[22] Interestingly, the results for IAF objectivity do not suggest any difference in IAF objectivity between misconduct firms and control firms during the misconduct years, or any incremental increase in IAF objectivity for the misconduct firms in the post-misconduct years compared to the misconduct years.

Overall, the analyses provide consistent evidence that IAF quality and competence are negatively associated with the likelihood of management misconduct and that misconduct firms increase IAF quality and competence after misconduct revelation. However, evidence from the multivariate levels analysis and the difference-in-differences analysis does not support the proposition that IAF objectivity deters management misconduct. Thus, IAF competence appears to be an important component of IAF quality with respect to deterring management misconduct.

VI. ADDITIONAL ANALYSIS

Accounting- and Nonaccounting-Related Management Misconduct

Internal auditors provide assurance related to business risks, not just accounting-related business risks. I next investigate separately the effects of IAF quality on accounting- and nonaccounting-related management misconduct. Accounting-related management misconduct, *ACCOUNTING_MIS*, includes (1) violation years from the Federal Securities Regulation Database where accounting fraud or other intentional accounting misconduct is alleged, unless the violation stems from a violation of the bribery provisions of the Foreign Corrupt Practices Act of 1977, and

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[21] In additional untabulated robustness tests, I find that the coefficients on IAF quality and competence remain negative and significant (1) after controlling for the extent to which the IAF is outsourced (2) after controlling for whether a firm is listed on the New York Stock Exchange, (3) for the banking industry subsample, and (4) for the subsample that excludes the banking industry.

[22] In untabulated results, the coefficient on the interaction of *MISCONDUCT_FIRM* and *POST_MISCONDUCT* is positive and significant when the dependent variable is *CERTIFICATION* or *TRAINING*. For *EXPERIENCE*, the coefficient is positive and has a one-tailed p-value of less than 11 percent. Thus, results are consistent with misconduct firms hiring more certified internal auditors and increasing training in response to revealed management misconduct.

TABLE 4

Misconduct Firm Response to Revealed Management Misconduct

$IAF\ QUALITY = \beta_0 + \beta_1 MISCONDUCT\_FIRM + \beta_2 POST\_MISCONDUCT + \beta_3 MISCONDUCT\_FIRM * POST\_MISCONDUCT + Controls + \varepsilon$

Independent Variables	**Pred. Sign**	*QUALITY*	*COMPETENCE*	*OBJECTIVITY*
MISCONDUCT_FIRM	–	–0.5697***	–0.2607**	0.0344
		(0.152)	(0.126)	(0.137)
POST_MISCONDUCT	?	–0.2765*	–0.2152	0.0578
		(0.168)	(0.143)	(0.135)
*MISCONDUCT_FIRM * POST_MISCONDUCT*	+	0.7207***	0.4221**	–0.2765
		(0.224)	(0.184)	(0.184)
ASSISTS_AUDITOR	?	1.2159	1.9853**	–0.5247
		(1.225)	(1.003)	(1.143)
AC_ACCOUNTING_EXP	+	–0.0527	–0.0923	0.2295
		(0.344)	(0.241)	(0.202)
BRD_PCT_IND	+	–0.0302	0.0441	0.4945
		(0.736)	(0.622)	(0.555)
CEOISCHAIR	–	–0.1910*	–0.0322	–0.2307**
		(0.138)	(0.105)	(0.107)
CEOFOUNDER	–	0.2024	0.1245	0.3941**
		(0.284)	(0.237)	(0.160)
INSTITUTIONAL_OWN	+	0.1416	0.0737	0.0193
		(0.251)	(0.193)	(0.160)
AUDITOR_SPECIALIST	?	0.0791	–0.0145	–0.0030
		(0.137)	(0.106)	(0.102)
LMARKETCAP	?	–0.1395***	–0.1118***	–0.1404***
		(0.052)	(0.036)	(0.044)
BM	?	0.1914	0.1987*	–0.0488
		(0.163)	(0.117)	(0.085)
LEVERAGE	?	0.4323	0.4330	0.0034
		(0.553)	(0.327)	(0.261)
PCTFORSALES	?	–0.1331	–0.2420	0.2373
		(0.307)	(0.234)	(0.202)
ISSUE	?	–0.4397***	–0.4641***	–0.0595
		(0.162)	(0.137)	(0.113)
Year Fixed Effects?		Yes	Yes	Yes
Industry Fixed Effects?		Yes	Yes	Yes
Total Obs.		358	358	358
Adjusted R^2		0.29	0.24	0.23

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*, **, *** Denote statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a one-tailed test where the coefficient sign is consistent with the predicted direction.

This table reports the results of an ordinary least squares regression examining the response of misconduct firms, as it relates to IAF quality, to revealed management misconduct as compared to a set of control firms. Violation and post-violation firm-years from misconduct firms are matched to firm-years from non-misconduct firms based upon year, industry, and closest market capitalization. The final sample includes 179 firm-years from misconduct firms, of which 105 are from misconduct years and 74 are from post-misconduct years, and a matched set of 179 firm-years from firms that never had misconduct during the sample period. Utilizing a difference-in-differences design, the response by misconduct firms to revealed misconduct is estimated based upon the following model: $QUALITY = \beta_0 + \beta_1 MISCONDUCT\_FIRM + \beta_2 POST\_MISCONDUCT + \beta_3 MISCONDUCT\_FIRM * POST\_MISCONDUCT + Controls + \varepsilon$.

(continued on next page)

TABLE 4 (continued)

MISCONDUCT_FIRM is an indicator variable that is set to 1 for observations from a firm that had misconduct during the sample period, and 0 otherwise. For example, if firm *i* had misconduct for years 2003 and 2004 and was in the sample from 2000 through 2009, then *MISCONDUCT_FIRM* would equal 1 for firm-years 2003 through 2009, and firm-years 2000 through 2002 would be omitted as they are prior to the misconduct. *POST_MISCONDUCT* is an indicator variable that is set to 1 for firm-years from misconduct firms that are subsequent to the violation years (i.e., where $MISCONDUCT = 0$ for year t, but $MISCONDUCT = 1$ for year $t-x$) and for matched firm-years from the control sample. Column (1) estimates the effect on IAF quality, column (2) estimates the effect on IAF competence, and column (3) estimates the effect on IAF objectivity.
Standard errors clustered by firm are in parentheses.
Variable definitions are provided in Appendix A.

(2) class period years for settled securities class-action lawsuits that allege violations of Generally Accepted Accounting Principles. I exclude violations of the bribery provisions of the Foreign Corrupt Practices Act of 1977 from accounting-related management misconduct and instead classify them as nonaccounting-related management misconduct, because after reading the individual cases, it is clear that the accounting manipulations were made in order to cover up the illegal bribery. In addition to these bribery violation years, nonaccounting-related management misconduct, *NONACCOUNTING_MIS*, includes class period years for settled securities class-action lawsuits that do not allege violations of Generally Accepted Accounting Principles.

The first and second (third and fourth) columns of Table 5 present the results for accounting- (nonaccounting-) related management misconduct.[23] Similar to the main analysis, the coefficient on *QUALITY* is negative and significantly associated with both types of misconduct. *COMPETENCE* is negatively associated nonaccounting-related misconduct, but not associated with accounting-related misconduct. *OBJECTIVITY* is unrelated to the likelihood of both types of misconduct. These results are consistent with the proposition that IAF quality deters accounting- and nonaccounting-related management misconduct. The negative relation between IAF quality and nonaccounting-related management misconduct is important because it is possible that the IAF is the main resource for audit committees to monitor nonaccounting management misconduct.

Predicting Accounting-Related Management Misconduct

The main analysis does not control for observable *indications* of financial misreporting (e.g., unusual accrual activity), which would assist in predicting concurrent accounting-related management misconduct. Next, I examine whether IAF quality serves as a predictor of accounting-related management misconduct. This provides insight into whether disclosures of IAF quality would be informative to investors. Interviewees in Archambeault et al. (2008) state that a report on internal audit would not only be useful, but could also increase IAF accountability and incentives to perform effectively. Holt and DeZoort (2009) found that experimental participants were more confident in financial reporting reliability and perceived the oversight of financial reporting to be more effective when provided with an internal audit report.

Dechow et al. (2011) develop an F-score, which predicts concurrent material accounting misstatements. I utilize Model 3 from Dechow et al. (2011) to calculate an F-score for each firm-year in the sample, excluding the finance industry.[24] I then test whether IAF quality is negatively associated with accounting-related management misconduct after controlling for the F-score. Table 6

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[23] In the accounting- (nonaccounting-) related regressions, the nonaccounting (accounting) misconduct observations are omitted. Inferences remain the same if all observations are included.

[24] Nonaccounting misconduct observations are also omitted. Inferences remain the same if these are included.

TABLE 5

Effect of IAF Quality on Accounting- and Nonaccounting-Related Management Misconduct

$Prob(MISCONDUCT = 1) = F(\beta_0 + \beta_1 QUALITY + Controls)$

Independent Variables	Pred. Sign	*ACCOUNTING_MIS* (1)	*ACCOUNTING_MIS* (2)	*NONACCOUNTING_MIS* (3)	*NONACCOUNTING_MIS* (4)
QUALITY	–	−0.2452**		−0.4489**	
		(0.142)		(0.239)	
COMPETENCE	–		−0.2653		−0.3543*
			(0.220)		(0.233)
OBJECTIVITY	–		0.0636		0.0278
			(0.194)		(0.178)
ASSISTS_AUDITOR	?	−0.7044	−0.5519	−6.2678	−6.0404
		(3.687)	(3.856)	(5.257)	(5.251)
AC_ACCOUNTING_EXP	–	−0.4158	−0.4325	−3.6362***	−3.7822***
		(0.870)	(0.888)	(1.344)	(1.380)
BRD_PCT_IND	–	−3.9727**	−4.0190**	2.4284	2.7092
		(2.050)	(2.042)	(3.089)	(3.280)
CEOISCHAIR	+	0.3072	0.3583	0.2531	0.3668
		(0.491)	(0.482)	(0.404)	(0.415)
CEOFOUNDER	+	1.4090**	1.3821**	0.4042	0.5248
		(0.787)	(0.778)	(1.010)	(0.913)
INSTITUTIONAL_OWN	–	−1.5666**	−1.5694**	−1.4748*	−1.3457*
		(0.700)	(0.708)	(0.958)	(0.914)
AUDITOR_SPECIALIST	–	−0.2781	−0.2847	0.4107	0.4126
		(0.439)	(0.435)	(0.508)	(0.510)
LMARKETCAP	+	0.2538**	0.2666**	0.4380**	0.4685**
		(0.153)	(0.151)	(0.202)	(0.220)
BM	–	0.3649	0.4281	0.6019***	0.6774***
		(0.369)	(0.373)	(0.196)	(0.196)
LEVERAGE	+	2.1677**	2.4050**	0.2741	0.5066
		(1.120)	(1.129)	(1.681)	(1.686)
PCTFORSALES	+	1.8810**	1.8985**	−0.5183	−0.5600
		(1.120)	(1.146)	(1.306)	(1.310)
ISSUE	+	0.1647	0.1618	−0.1056	−0.0167
		(0.398)	(0.402)	(0.593)	(0.580)
Year Fixed Effects?		Yes	Yes	Yes	Yes
Industry Fixed Effects?		Yes	Yes	Yes	Yes
Obs. where *MISCONDUCT* = 1		63	63	44	44
Total Obs.		1,354	1,354	1,335	1,335
Pseudo R^2		0.35	0.35	0.41	0.40

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*, **, *** Denote statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a one-tailed test where the coefficient sign is consistent with the predicted direction.

This table reports the results of a logistic regression examining the effect of IAF quality on the likelihood of accounting- and nonaccounting-related management misconduct based on the following model: $Prob(MISCONDUCT = 1) = F(\beta_0 + \beta_1 QUALITY + Controls)$.

The accounting-related misconduct sample (*ACCOUNTING_MIS*) includes (1) violation years from the Federal Securities Regulation Database where fraud or other intentional misconduct is alleged, unless the violation stems from a violation of the bribery provision of the Foreign Corrupt Practices Act of 1977, and (2) class period years for settled securities class-action lawsuits that allege violations of Generally Accepted Accounting Principles per the Stanford Securities Class Action Clearinghouse. Results for accounting-related management misconduct are in columns (1) and

(continued on next page)

TABLE 5 (continued)

(2). The nonaccounting-related misconduct sample (*NONACCOUNTING_MIS*) includes (1) class period years for settled securities class-action lawsuits that do not allege violations of Generally Accepted Accounting Principles per the Stanford Securities Class Action Clearinghouse, and (2) violation years for SEC and Department of Justice Enforcement Actions claiming violation of the bribery provisions of the Foreign Corrupt Practices Act of 1977. Results for nonaccounting-related management misconduct are in columns (3) and (4). In columns (2) and (4), the composite IAF quality measure is replaced with *COMPETENCE* and *OBJECTIVITY*.
Standard errors clustered by firm are in parentheses. Pseudo R^2 is the max re-scaled R^2 from SAS (also known as Cragg and Uhler's pseudo R^2).
Variable definitions are provided in Appendix A.

presents the results. In the first column, I regress *ACCOUNTING_MIS* on *QUALITY* and *FSCORE*. The coefficient on *FSCORE* is positive and significant, as expected. The coefficient on *QUALITY* is negative and significant, suggesting that IAF quality is a predictor of accounting-related management misconduct even after controlling for the F-score. In column (2), I include the control variables from Equation (1) in the regression with the exception of *ISSUE*, as it is part of the F-score calculation. For the third column, F-score is replaced with the underlying variables used to calculate the F-score, and in the fourth column, control variables from Equation (1) are included. Consistent with column (1), the coefficients on *QUALITY* in columns (2), (3), and (4) are negative and significant. These results suggest that IAF quality is a good predictor of accounting-related management misconduct and provide empirical evidence of the claims of stakeholders in Archambeault et al. (2008) that disclosure of IAF quality could be beneficial to the market.

VII. CONCLUSION

Standard-setters claim IAF quality deters management misconduct. In response to management misconduct in the early 2000s, regulators passed regulation that enforces board, audit committee, and external auditor quality, but did not pass regulation that enforces IAF quality or requires IAF-related disclosures. I find that a composite measure of IAF quality, as well as IAF competence, is negatively associated with the likelihood of management misconduct, and that misconduct firms improve IAF quality through IAF competence after misconduct revelations. These results suggest that regulators and audit committees should consider ways to improve IAF quality and, more specifically, IAF competence. For example, firms listed on the New York Stock Exchange are required to have an IAF that meets privately with the audit committee, and the NASDAQ is currently considering similar requirements. Access to the audit committee could improve IAF objectivity, but listing requirements do not directly address IAF competence. My study suggests that these stock exchanges should also consider ways to ensure listed firms have competent IAFs.

I also find that IAF quality is negatively associated with accounting-related management misconduct even after controlling for observable predictors of misconduct (Dechow et al. 2011). This suggests that IAF quality is a good predictor of accounting-related management misconduct and supports survey and experimental evidence that reports that IAF-related disclosures could affect investment decisions (Archambeault et al. 2008; Holt and DeZoort 2009). Accordingly, investors could benefit from disclosures pertaining to IAF quality.

There are several limitations to this study. First, it is possible that IAF quality and the likelihood of management misconduct are jointly determined. Because of this, I include control variables in the levels analysis that capture corporate governance quality. Additionally, the changes analysis helps to allay some endogeneity concerns because the changes evidence is consistent with firms viewing improving IAF quality as an important response to management misconduct. Second, the lack of a relation between IAF objectivity and the likelihood of management misconduct should

TABLE 6

Predictive Ability of IAF Quality on Accounting-Related Management Misconduct

$Prob(ACCOUNTING\_MIS = 1) = F(\beta_0 + \beta_1 QUALITY + Controls)$

Independent Variables	Pred. Sign	(1)	(2)	(3)	(4)
QUALITY	–	−0.4967***	−0.3248*	−0.5014***	−0.2606*
		(0.160)	(0.209)	(0.155)	(0.201)
FSCORE	+	12.1356***	12.8779***		
		(2.336)	(4.237)		
ASSISTS_AUDITOR	?		0.6284		1.6324
			(5.244)		(5.257)
AC_ACCOUNTING_EXP	–		−0.1429		−0.4110
			(1.292)		(1.309)
BRD_PCT_IND	–		−4.4607*		−5.1009**
			(2.960)		(2.655)
CEOISCHAIR	+		0.2847		0.2933
			(0.649)		(0.686)
CEOFOUNDER	+		0.7753		1.1860
			(1.058)		(1.104)
INSTITUTIONAL_OWN	–		−1.8926**		−2.1390***
			(0.876)		(0.878)
AUDITOR_SPECIALIST	–		−0.4211		−0.5494
			(0.525)		(0.530)
LMARKETCAP	+		0.1242		0.2119
			(0.170)		(0.166)
BM	–		−0.3714		−0.6137**
			(0.407)		(0.294)
LEVERAGE	+		−0.3832		−0.9273
			(1.802)		(1.652)
PCTFORSALES	+		2.5862**		2.8535**
			(1.375)		(1.468)
RSST_ACC	+			1.5658	0.4166
				(1.573)	(2.290)
CH_REC	+			4.5298	5.3012
				(5.605)	(6.048)
CH_INV	+			−11.0665	−0.4803
				(6.781)	(9.635)
SOFT_ASSETS	+			0.6683	3.6692***
				(0.860)	(1.466)
CH_CS	+			0.6541**	0.5166*
				(0.288)	(0.334)
CH_ROA	–			−2.8358	−4.8722*
				(2.448)	(3.434)
ISSUE	+			0.2949	0.4891
				(0.502)	(0.533)
CH_EMP	–			−0.5613	0.5405
				(0.567)	(0.872)
LEASE	+			−0.2442	1.9693**
				(0.556)	(0.901)

(continued on next page)

TABLE 6 (continued)

Independent Variables	Pred. Sign	(1)	(2)	(3)	(4)
$RETURN_t$	+			−2.6228*** (0.744)	−2.8900*** (1.018)
$RETURN_{t-1}$	+			−0.1454 (0.511)	0.1256 (0.424)
Year Fixed Effects?		No	Yes	No	Yes
Industry Fixed Effects?		No	Yes	No	Yes
Obs. where $MISCONDUCT = 1$		42	42	42	42
Total Obs.		1,022	1,022	1,022	1,022
Pseudo R^2		0.15	0.41	0.16	0.46

*, **, *** Denote statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a one-tailed test where the coefficient sign is consistent with the predicted direction.

This table reports the results of a logistic regression examining the effect of IAF quality on the likelihood of accounting-related management misconduct based on the following model: $Prob(ACCOUNTING\_MIS = 1) = F(\beta_0 + \beta_1 QUALITY + Controls)$.

The accounting-related misconduct sample includes (1) violation years from the Federal Securities Regulation Database where fraud or other intentional misconduct is alleged, unless the violation stems from a violation of the bribery provision of the Foreign Corrupt Practices Act of 1977, and (2) class period years for settled securities class-action lawsuits that allege violations of Generally Accepted Accounting Principles per the Stanford Securities Class Action Clearinghouse. In column (1), I include the F-score based upon Dechow et al. (2011) as a control variable. In column (2), I include the F-score and control variables from Equation (1). In column (3), I include the underlying variables from Dechow et al. (2011) that are used to calculate the F-score. In column (4), I include control variables from Equation (1) and the underlying variables from Dechow et al. (2011) that are used to calculate the F-score.

Standard errors clustered by firm are in parentheses. Pseudo R^2 is the max re-scaled R^2 from SAS (also known as Cragg and Uhler's pseudo R^2).

Variable definitions are provided in Appendix A.

be interpreted with caution. It is possible that all firms in the sample have sufficiently high levels of IAF objectivity and/or that there is not enough variation in the IAF objectivity measures to demonstrate a significant association. Future studies could reexamine this relation as better measures of IAF objectivity become available. Third, it is possible that survey respondents introduced errors in the data. While errors should introduce noise rather than biases, the reader should consider the potential for errors in the survey responses.

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APPENDIX A

Variable Definitions

Variable	Definition
Dependent Variables	
MISCONDUCT	An indicator variable equal to 1 for (1) violation years from the Federal Securities Regulation Database where fraud or other intentional misconduct is alleged, (2) class period years for settled securities class-action lawsuits per the Stanford Securities Class Action Clearinghouse, and (3) violation years for SEC and Department of Justice Enforcement Actions claiming violation of the bribery provisions of the Foreign Corrupt Practices Act.
ACCOUNTING_MIS	An indicator variable equal to 1 for (1) violation years from the Federal Securities Regulation Database where accounting fraud or other intentional accounting misconduct is alleged, unless the violation stems from a violation of the bribery provision of the Foreign Corrupt Practices Act, and (2) class period years for settled securities class-action lawsuits that allege violations of Generally Accepted Accounting Principles per the Stanford Securities Class Action Clearinghouse.
NONACCOUNTING_MIS	An indicator variable equal to 1 for (1) class period years for settled securities class-action lawsuits that do not allege violations of Generally Accepted Accounting Principles per the Stanford Securities Class Action Clearinghouse, and (2) violation years for SEC and Department of Justice Enforcement Actions claiming violation of the bribery provisions of the Foreign Corrupt Practices Act.
Internal Audit Variables—all data come from the Global Auditing Information Network and Compustat databases. Compustat variables names are in parentheses.	
QUALITY	A composite measure of internal audit function quality. The variable can range from 0 to 6, with 0 representing lowest quality and 6 representing highest quality. The score is formed by summing *EXPERIENCE_IND*, *CERTIFICATION_IND*, *TRAINING_IND*, *NOT_TRAINING_GROUND*, *INDEPENDENT*, and *IAF_SIZE_IND*.
COMPETENCE	A factor score derived from a principal-component factor analysis of standardized measures of *EXPERIENCE*, *CERTIFICATION*, *TRAINING*, *NOT_TRAINING_GROUND*, *INDEPENDENT*, and *IAF_SIZE*.
OBJECTIVITY	A factor score derived from a principal-component factor analysis of standardized measures of *EXPERIENCE*, *CERTIFICATION*, *TRAINING*, *NOT_TRAINING_GROUND*, *INDEPENDENT*, and *IAF_SIZE*.
EXPERIENCE	The average number of years of internal audit experience for year *t*.
EXPERIENCE_IND	An indicator equal to 1 if *EXPERIENCE* is above the median, and 0 otherwise.
CERTIFICATION	The percentage of internal auditors who have CIA, CPA, or CFE certifications in year *t*.
CERTIFICATION_IND	An indicator equal to 1 if *CERTIFICATION* is above the median, and 0 otherwise.
TRAINING	The average number of training hours internal auditors attend in year *t*.

(continued on next page)

APPENDIX A (continued)

Variable	Definition
TRAINING_IND	An indicator equal to 1 if *TRAINING* is above the median, and 0 otherwise.
INDEPENDENT	An indicator that is equal to 1 if the chief audit executive functionally reports to the audit committee in year *t*, and 0 otherwise.
NOT_TRAINING_GROUND	An indicator equal to 1 if the internal audit function is not used as a management training ground during year *t*, and 0 otherwise.
IAF_SIZE	The average dollar amount spent on internal auditing for the industry in year *t* divided by the average total sales (SALE) of the industry in year *t*, subtracted from the dollar amount spent on internal auditing for firm *i* in year *t* divided by firm *i*'s total sales (SALE) in year *t*. This amount is then divided by the average dollar amount spent on internal auditing for the industry in year *t*, divided by the average total sales (SALE) of the industry in year *t*.
IAF_SIZE_IND	An indicator equal to 1 if *IAF_SIZE* is positive, and 0 otherwise.
ASSISTS_AUDITOR	The percentage of time the internal audit function spends assisting the external auditors in year *t*.
Variables for Difference-in-Differences Design	
MISCONDUCT_FIRM	An indicator variable coded 1 if the observation is from a firm that had misconduct during the sample period, and 0 otherwise. For example, if firm *i* had misconduct for years 2003 and 2004 and was in the sample from 2000 through 2009, then *MISCONDUCT_FIRM* would equal 1 for firm-years 2003 through 2009, and firm-years 2000 through 2002 would be excluded from the analysis as they are prior to the alleged misconduct.
POST_MISCONDUCT	An indicator variable that is set to 1 for firm-years from misconduct firms that are subsequent to the violation years (i.e., where *MISCONDUCT* = 0 for year *t*, but *MISCONDUCT* = 1 for year $t-x$) and for matched firm-years from the control sample.
Control Variables—data come from BoardEx, Compustat, Corporate Library, CRSP, and Thomson Reuters. Compustat variables names are in parentheses.	
AC_ACCOUNTING_EXP	Percentage of audit committee members with accounting expertise in year *t*. Data for this variable come from BoardEx. Accounting expertise is defined as work experience as a public auditor at one of the 25 audit firms listed in Compustat, as a CPA or Chartered Accountant, or in an accounting-specific position, such as CFO, Treasurer, Controller, Head of Accounting, etc.
BRD_PCT_IND	Percentage of directors that is independent in year *t*. Data for this variable come from BoardEx.
CEOISCHAIR	An indicator variable that is equal to 1 if the CEO is also the chair of the board of directors in year *t*, and 0 otherwise. Data for this variable come from BoardEx.
CEOFOUNDER	An indicator variable that is equal to 1 if the CEO in year *t* is also the founder of the firm, and 0 otherwise. Data for this variable come from BoardEx and Corporate Library.
INSTITUTIONAL_OWN	Percentage of shares outstanding held by institutional investors at the end of year *t*. Data for this variable come from Thomson Reuters and Compustat (CSHO).

(continued on next page)

APPENDIX A (continued)

Variable	Definition
AUDITOR_SPECIALIST	An indicator variable that is equal to 1 if the external auditor has the highest industry market share for year *t*, and 0 otherwise. Industry market share is calculated as the amount of annual sales revenue (SALE) audited in year *t* for a specific industry.
LMARKETCAP	Natural log of market capitalization (PRCC_F $*$ CSHO) at the end of year *t*.
BM	Book value of common equity (CEQ) divided by the market value of common equity (PRCC_F $*$ CSHO) at the end of year *t*.
LEVERAGE	Total debt (DLTT + DLC) divided by total assets (AT) at the end of year *t*.
PCTFORSALES	Percentage of total sales (SALE) attributable to foreign segments in year *t* as reported in the Compustat Segments database.
ISSUE	An indicator variable coded 1 if the sum of issued securities (SSTK > 0 or DLTIS > 0) during year *t* is greater than 10 percent of total assets (AT) at the end of year *t*, and 0 otherwise.
RSST_ACC	(ΔWC + ΔNCO + ΔFIN)/Average total assets for year *t*, where WC = [Current Assets (ACT) − Cash and Short-Term Investments (CHE)] − [Current Liabilities (LCT) − Debt in Current Liabilities (DLC)]; NCO = [Total Assets (AT) − Current Assets (ACT) − Investments and Advances (IVAO)] − [Total Liabilities (LT) − Current Liabilities (DCT) − Long-Term Debt (DLTT)]; FIN = [Short-Term Investments (IVST) + Long-Term Investments (IVAO)] − [Long-Term Debt (DLTT) + Debt in Current Liabilities (DLC) + Preferred Stock (PSTK)]; following Richardson, Sloan, Soliman, and Tuna (2005).
CH_REC	ΔAccounts Receivable (RECT)/Average Total Assets for year *t*.
CH_INV	ΔInventory (INVT)/Average Total Assets for year *t*.
SOFT_ASSETS	(Total Assets (AT) − PP&E (PPENT) − Cash and Cash Equivalent (CHE))/Total Assets (AT) at the end of year *t*.
CH_CS	Percentage change in cash sales [Sales (SALE) − ΔAccounts Receivable (RECT)] for year *t*.
CH_ROA	[Earnings$_t$ (IB)/Average total assets$_t$] − [Earnings$_{t-1}$/Average total assets$_{t-1}$].
CH_EMP	Percentage change in the number of employees (EMP)/percentage change in assets (AT) for year *t*.
LEASE	An indicator variable coded 1 if future operating lease obligations (mrc1, mrc2, mrc3, mrc4, and mrc5) are greater than 0 for year *t*, and 0 otherwise.
RETURN	Annual buy-and-hold return inclusive of delisting returns minus the annual buy-and-hold value-weighted market return for year *t*.
FSCORE	Equals $-4.2388 + 1.2671\mathit{RSST\_ACC}_t + 5.5275\mathit{CH\_REC}_t - 11.1314\mathit{CH\_INV}_t + 1.0695\mathit{SOFT\_ASSETS}_t + 0.5511\mathit{CH\_CS}_t - 2.7419\mathit{CH\_ROA}_t + 0.2947\mathit{ISSUE}_t - 0.5602\mathit{CH\_EMP}_t - 0.4097\mathit{LEASE}_t - 2.4602\mathit{RETURN}_t - 0.5417\mathit{RETURN}_{t-1}$. For empirical tests, *FSCORE* is converted to a probability using the following formula: exp(*FSCORE*)/(1 + exp(*FSCORE*)).

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Does Ineffective Internal Control over Financial Reporting affect a Firm's Operations? Evidence from Firms' Inventory Management

Mei Feng
Chan Li
University of Pittsburgh

Sarah E. McVay
University of Washington

Hollis Skaife
University of California, Davis

ABSTRACT: We investigate whether ineffective internal control over financial reporting has implications for firm operations by examining the association between inventory-related material weaknesses in internal control over financial reporting and firms' inventory management. We find that firms with inventory-related material weaknesses have systematically lower inventory turnover ratios and are more likely to report inventory impairments relative to firms with effective internal control over financial reporting. We also find that inventory turnover rates increase for firms that remediate material weaknesses related to inventory tracking. Remediating firms also experience increases in sales, gross profit, and operating cash flows. Finally, we assess the generalizability of our findings by examining all material weaknesses in internal control over financial reporting, regardless of type, and provide evidence that firms' returns on assets are associated with both their existence and remediation. Collectively, our findings support the general hypothesis that internal control over financial reporting has an economically significant effect on firm operations.

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I. INTRODUCTION

For the past decade, managers and regulators have debated the costs and benefits of Section 404 of the Sarbanes-Oxley Act (U.S. House of Representatives 2002), which requires disclosure of the effectiveness of internal control over financial reporting (ICFR). A number of research studies document negative consequences of maintaining ineffective ICFR, such as lower earnings quality, higher cost of equity and debt, higher management and analyst forecast errors, and inefficient investments.[1] Following the disclosure of ineffective ICFR, firms tend to remediate material weaknesses, and prior research documents economically significant improvements in some areas, such as financial reporting quality (Ashbaugh-Skaife, Collins, Kinney, and LaFond 2008). Consistent with these improvements, a recent survey reveals that most managers believe that Section 404 has improved the quality of their financial reporting. However, the average manager does not believe that the regulation has improved the efficiency of their firms' operations (Alexander, Bauguess, Bernile, Lee, and Marietta-Westberg 2013). In other words, managers appear to recognize that they provide higher-quality information as a result of effective ICFR, but they do not appear to recognize that they may also be *using* higher-quality information to make better operational decisions when they maintain effective ICFR because some controls play both operational and financial reporting roles. The purpose of our study is to assess the implications of ICFR for firm operations.[2]

We explore the effect of inventory-related material weaknesses in internal control (MWIC) on firm operations because inventory is a critical component of firm strategy. Having inventory available the moment it is demanded by the customer or required for production, with the proper specifications, and at the best cost for the desired quality, sustains firm operations. When a firm lacks consistent policies related to the accounting for routine inventory transactions, there can be greater variation in unit costs, invoice payment dates, and shipping fees, thereby increasing inventory costs. Moreover, lax policies and procedures related to inventory tracking and valuation can result in substantial lost sales due to inventory shortages, as well as increased capital charges and risk of obsolescence due to inventory surpluses. In summary, when a firm fails to have adequate systems to control inventory purchase, tracking, and valuation, there is a greater likelihood of a mismatch between inventory supply and demand, leading to poorer operating performance.[3]

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[1] Examples include Doyle, Ge, and McVay (2007a); Ashbaugh-Skaife, Collins, Kinney, and LaFond (2009); Feng, Li, and McVay (2009); Chen, Lai, and Liu (2011); Costello and Wittenberg-Moerman (2011); Dhaliwal, Hogan, Trezevant, and Wilkins (2011); Li, Peters, Richardson, and Watson (2012); Cheng, Dhaliwal, and Zhang (2013); and Clinton, Pinello, and Skaife (2014).

[2] The Committee of Sponsoring Organization's (COSO) *Internal Control—Integrated Framework* defines internal control as a process that is designed to provide reasonable assurance that a firm can achieve its objectives (COSO 1992). The COSO *Framework* distinguishes between reporting objectives and operating objectives, and describes elements of internal control that enhance the reliability of financial reporting versus the effectiveness and efficiency of operations, respectively. Many of the same policies, procedures, and controls that lead to effective ICFR related to inventory, however, also affect the proper accounting for inventory acquisition, sale, storage, and returns, leading to more effective inventory management. Thus, we expect there to be mutually beneficial effects on financial reporting quality (Doyle, Ge, and McVay 2007b) and firms' inventory management as a result of having effective ICFR.

[3] This is distinct from prior research that provides evidence that poor operating performance is a *determinant* of ineffective ICFR. We conjecture that poor performance is also a possible *consequence* of having ineffective ICFR conditional on current performance. We control for firms' contemporaneous operating profitability in our analyses and conduct change analyses to further distinguish causality. We also conduct a within-performance-levels analysis as a robustness check in Section IV.

We use inventory turnover and inventory impairments, two commonly cited accounting-based measures, to assess the relation between inventory-related MWIC and inventory management. Inventory turnover is often used to assess managerial performance and is a common component of any industry competitive analysis (Easton 2009). Holding profit margins constant, lower inventory turnover ratios, defined as cost of goods sold divided by average inventory, are associated with less profitable operations (Huson and Nanda 1995). Firms having inventory-related MWIC potentially lack the proper policies and procedures to handle and record inventory transactions, thereby placing suboptimal order quantities that culminate in higher cost of goods sold via higher holding or stock-out costs. As a result, we expect firms with inventory-related MWIC to have lower inventory turnover ratios relative to firms with effective ICFR. Likewise, if inaccurate inventory tracking and errors in the internal valuation process result in firms not managing their inventory properly, then we expect firms with inventory-related MWIC to have larger and more frequent inventory impairments as out-of-date or obsolete product loses market value.[4] Finding no relation between inventory-related MWIC and inventory turnover or impairments would be consistent with either (1) managers relying on internal data from a different system to make operational decisions, or (2) the errors introduced to the system as a result of inventory-related MWIC not being economically significant. As such, documenting that inventory-related MWIC affect inventory management would provide an important link between ICFR and firm operations that has not been documented to date.

Among our sample firms that report MWIC, more than 25 percent (161/640) report MWIC that relate to inventory.[5] Descriptive statistics indicate that inventory turnover is significantly lower, on average, for inventory-related MWIC firms (8.37 or 43 days) relative to the inventory turnover of firms with effective ICFR (14.03 or 26 days) and of firms with other types of MWIC (14.34 or 25 days). The significantly slower inventory turnover for firms with inventory-related MWIC is substantiated in our multivariate analysis, where we control for revenue-related and other types of material weaknesses, as well as factors related to inventory turnover and determinants of MWIC. Using the information from the Section 404 reports, we also partition inventory-related MWIC into tracking- and valuation-related issues, where the former are those weaknesses that could affect the recorded existence or quantity of inventory and the latter are those weaknesses that could affect the recorded value of inventory; we provide examples in Section IV. We find significantly lower inventory turnover ratios for both. Examining post-remediation performance, we find evidence that inventory turnover ratios increase after firms remediate their inventory-tracking MWIC. Importantly, we do not find an increase in inventory turnover ratios following the remediation of other types of MWIC.

Our second analysis focuses on inventory impairments. Inventory impairments are an attractive measure to assess inventory management because the link to firm performance is direct and immediate, as inventory write-offs increase current-period expenses, resulting in lower earnings. We find that firms with inventory-related MWIC are (1) more likely to report inventory impairments, and (2) report significantly larger inventory impairments. These findings hold for both inventory tracking- and valuation-related MWIC. The results of the inventory turnover and inventory impairment tests provide evidence that ineffective ICFR related to inventory has adverse consequences for inventory management, leading to less profitable operations.

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[4] As we discuss below, inventory values reported in firms' 10-Ks should not be unduly affected by the ineffective ICFR because they have been corrected with a physical count and costing of the ending inventory units via substantive audit procedures. U.S. Generally Accepted Accounting Principles (GAAP) require inventory to be reported at the lower of cost or market, which drives the recognition of inventory impairments and increases the reliability of the ending inventory value reported on the balance sheet. Moreover, none of the firms in the sample reporting inventory-related MWIC received qualified audit opinions from their auditors due to the inability of the auditor to opine on the inventory value reported on the balance sheet. We expect the day-to-day inventory quantities and values to be less reliable when firms have inventory-related MWIC. Less reliable inventory quantities and values for internal reporting purposes hamper day-to-day decision making, thereby leading to poorer inventory management and less profitable operations.

[5] We focus on accelerated filers because this group of firms has been subject to both Section 404(a) and 404(b) since 2004. See Section III for more details.

To corroborate this conclusion, we investigate changes in firms' operating performance following the remediation of inventory-related MWIC. If effective inventory-related ICFR leads to better inventory management, then we expect operating performance to improve once firms remediate their inventory-related MWIC. We find that firms that correct their inventory-related MWIC report significant increases in sales, gross margin, and operating cash flows after remediation. Moreover, once firms remediate their inventory-related MWIC, we find that their gross profit and operating income are no longer significantly different from firms with effective ICFR.

We conclude our study with a broader examination of the association between all MWIC and a firm's return on assets (ROA). We document that the average return on assets is lower for firms with MWIC and that the remediation of MWIC is associated with higher future return on assets. These findings, combined with the results of our inventory-related MWIC analyses, support our general hypothesis that ICFR has an economically significant effect on firm operations.

Our study makes several contributions to the literature. First, our study extends the literature on the consequences of having ineffective ICFR. A number of research studies provide evidence that information disseminated from firms disclosing ineffective ICFR is of lower quality than information from other firms. Examples include reported earnings or forecasted earnings (Doyle et al. 2007a; Ashbaugh-Skaife et al. 2008; Feng et al. 2009; Altamuro and Beatty 2010; Li et al. 2012). Our study focuses on whether management *acts on* lower-quality information produced from a system with ineffective ICFR when making operational decisions, thereby affecting firm performance. We document an explicit link between inventory-related MWIC and poor inventory management, as well as substantiate that inventory management improves once inventory-related MWIC are remediated. Further, we provide evidence on the generalizability of our results by examining all material weaknesses, regardless of type, and provide evidence that firms' returns on assets are associated with both their existence and remediation. We are the first paper to examine the broad effect of ineffective ICFR on firm operations, and to establish a more direct link between MWIC over inventory and managers' inventory management decisions. These results provide strong evidence that despite being largely unremarked upon as a potential benefit by managers or regulators, maintaining effective ICFR can provide an economically meaningful benefit to their firms' operations. To the extent that there is a disconnect between actual and perceived benefits to maintaining effective ICFR, the recent regulatory move to exempt certain firms from internal control disclosure regulation may be premature.

Our study also contributes to the literature on inventory management and firm operations. Most of the prior literature on inventory management relies on insights obtained via case studies, surveys, and questionnaires to document the consequences of inventory mismanagement (e.g., Anderson, Fitzsimons, and Simester 2006). For a large sample of publicly traded firms, we provide evidence that the lack of proper inventory acquisition, tracking, or valuation systems has a direct impact on firms' operating performance.

II. HYPOTHESIS DEVELOPMENT

Internal control is a process that is designed to provide reasonable assurance that a firm can achieve its objectives, where differing aspects of internal control can be partitioned into operating objectives, reporting objectives, and compliance objectives (COSO 1992). Internal control over operations is comprised of policies, procedures, and personnel intended to enhance the effectiveness and efficiency of firm operations and safeguard assets (COSO 2013; Lawrence, Minutti-Meza, and Vyas 2014). Internal control over financial reporting (ICFR) encompasses the processes and procedures established by management to maintain records that accurately reflect the firm's transactions (Deloitte & Touche, Ernst & Young, KPMG, and PricewaterhouseCoopers 2004). The development of policies and procedures related to effective ICFR is a strategic choice by management and boards of directors, who weigh the costs and benefits of establishing and maintaining effective ICFR (COSO 2013), as well as the costs of disclosing ineffective ICFR (Government Accountability Office [GAO] 2013).

ICFR became a well-known component of internal control as a result of Section 404 of the Sarbanes-Oxley Act (SOX) of 2002, which requires both managers and auditors to assess and publicly disclose the effectiveness of ICFR. Accelerated filers began complying with the rule in 2004, whereas the compliance date for non-accelerated filers was delayed until 2007 and, under the Dodd-Frank Act (U.S. House of Representatives 2010), only requires management's assessment.

Since the implementation of Section 404, a vast literature on ICFR has emerged. Much of this literature focuses on how ineffective ICFR affects investors' resource allocation decisions (Beneish, Billings, and Hodder 2008; Costello and Wittenberg-Moerman 2011; Skaife and Wangerin 2013), firms' cost of capital (Ashbaugh-Skaife et al. 2009; Dhaliwal et al. 2011), or analysts' behavior (Clinton et al. 2014). A number of research studies document negative consequences to maintaining ineffective ICFR; for example, diminished quality of external financial reporting (Doyle et al. 2007a; Ashbaugh-Skaife et al. 2008; Altamuro and Beatty 2010) and internal financial reports (Feng et al. 2009; Li et al. 2012). These studies provide evidence that information, such as reported earnings or forecasted earnings, disseminated from firms disclosing ineffective ICFR is of lower quality than information from other firms. The focus of our study is whether management *acts on* lower-quality information when making operational decisions, thereby affecting firm performance.

Recent studies have begun to explore the extent to which managers act on faulty information as a result of ineffective ICFR. Bauer (2014) posits that tax-related MWIC reflect management's capability or discretion for tax planning and finds evidence that firms with tax-related MWIC have higher effective tax rates than firms without tax-related MWIC. More broadly, Cheng et al. (2013) investigate the efficiency of firm investments, such as property, plant, and equipment (PP&E) and research and development (R&D), in the presence of ineffective ICFR, concluding that managers of firms with ineffective ICFR make poorer investments.[6] However, they do not examine the resulting performance, such as return on assets, and they do not conduct cross-sectional tests examining investment-specific weaknesses related to PP&E and R&D. Thus, their evidence is indirect.

Further, no studies examine the effect of MWIC on crucial day-to-day operating decisions made by management. A recent survey reveals that although the majority of managers believe that Section 404 has improved the quality of their internal control structure and improved the quality of their financial reporting, only 29 percent of managers believe that having effective internal control will have a positive impact on their operations (Alexander et al. 2013). One possible reason for such beliefs is that managers may rely on other information to make operating decisions, including inventory management decisions.[7] As a result, material weaknesses in ICFR may not have a spillover effect on the quality of the information that managers use for operational decisions.

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[6] Lawrence et al. (2014) examine privacy breaches, which are an indication of operational control risk. They document a positive leading relation between operational control risk and financial reporting control risk and, in turn, total audit risk. They suggest that known control risks, such as privacy breaches, are important for risk assessments and should be a required disclosure to regulators and external market participants. They also suggest that concerns about control risk have been overshadowed by Section 404's focus on ICFR. Our study suggests that some ICFR also have implications for control risk.

[7] For example, Procter & Gamble uses SAP for the company's financial reporting. However, many divisions and processes rely on legacy systems for daily operations. Procter & Gamble translates financial data from different divisions to the central SAP periodically. It is possible that those data are mistranslated as they are rolled up into the financial reporting system (S. Wright and A. Wright 2002). Therefore, even though the quality of the financial data is significantly diminished during this integration process, the data in the operational legacy systems that managers rely on to make business decisions are sound.

However, we expect that, often, the same systems are used for internal management and financial reporting and, thus, the additional disclosure regulation over ICFR will have positive spillover effects on managers' operational decisions. In particular, because Section 404 requires the formal assessment and disclosure of ICFR, these controls are brought to the attention of management when they might not otherwise have been discovered and, in addition, identified MWIC are often remediated. Prior research has documented an improvement in financial reporting quality following this remediation; we investigate whether MWIC and the related disclosure regulation also affect operational performance.

We examine whether an explicit link exists between the effectiveness of ICFR and operating performance by looking at the association between inventory-related MWIC and inventory management. The objective of inventory management is to provide sufficient inventory quantities to serve customers' or production needs while minimizing the sum of total variable inventory costs. Inventory-related MWIC would also affect inventory management if the same policies and procedures that contribute to effective inventory-related ICFR also affect inventory management decisions in the form of procurement, tracking, and pricing of inventory. For example, many retailers and manufacturers use automated inventory ordering systems that rely on recorded inventory quantities to trigger purchase orders. If inventory quantity is mistakenly under- (over-) reported because inventory is not tracked properly, then too many (too few) goods will be ordered, resulting in an overstock (stock-out) and additional holding (stock-out) costs (Nachtmann, Waller, and Rieske 2010). Having policies and procedures that lead to more accurate and timely inventory data, including quantity, location, and cost, helps firms manage their inventory more efficiently (Croson and Donohue 2006) and allow automated inventory systems to function properly (Chen 1999). In contrast, when a firm has inventory-related MWIC, inventory data for inventory management will be inaccurate, leading to suboptimal inventory management decisions. As an example of an inventory-related MWIC, consider the following excerpt from IPIX Corp.'s 2004 10-K:

> As of December 31, 2004, the Company did not maintain effective internal control over inventory ... These deficiencies were considered to be material weaknesses:
>
> - Controls to ensure inventory receipts are properly received and accounted for were not adequately documented or tested.
> - Controls relating to inventory counts and the reconciliations between accounting records and inventory counts were not adequately documented or tested. Specifically, the Company did not maintain documented inventory count instructions, and there was a lack of sufficient evidence that inventory reconciliations were performed and reviewed by appropriate personnel.
> - Controls relating to the proper costing and valuation of inventory were not adequately documented or tested.

This example illustrates that inventory-related MWIC affect information about the quantity and costing of inventory—important inputs to inventory management decisions such as purchasing and pricing (DeHoratius and Raman 2008).

When inventory is not properly tracked, larger deviations are likely between the inventory levels reported in the company's internal documents and the actual inventory quantities on hand.[8] Operating decisions based on these potentially faulty internal management reports will lead to mismatches between demand and supply (Hendricks and Singhal 2005). Hence, firms with inaccurate inventory records are more likely to hold excess inventory or incur inventory shortages.[9] Inventory shortages resulting from inaccurate records will result in lost sales, because sales are constrained by available inventory (Iglehart and Morey 1972; Anderson et al. 2006; Lai, Debo, and Nan 2011). Lost sales result in adverse consequences for budgeted sales and future inventory acquisitions because accounting records will not appropriately reflect customers' demand for products. Alternatively, inadvertently carrying inventory levels that are too high is also costly, as it ties up capital, increases holding costs, and increases the risk of obsolescence (Sheppard and Brown 1993; DeHoratius and Raman 2008). When obsolete inventory is identified and written off as a current-period expense, it reduces operating income.

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[8] Raman, DeHoratius, and Zeynep (2001) note that inventory error, defined as the difference between physical and system inventory levels, often enters automated inventory systems through human action, e.g., failing to properly record customer returns, misplacing inventory on the sales floor, or improper storage of inventory. For example, inventory errors can occur during the return process when a customer exchanges a garment for a different size and the retailer does not have standardized procedures for recording the exchange.

Importantly, we expect inventory-related MWIC to affect inventory transactions throughout the year more than they affect the year-end inventory balances reported on financial statements filed with the Securities and Exchange Commission (SEC). Financial statement errors resulting from inventory-related MWIC should be corrected by auditors' substantive testing during the external audit (Doss and Jonas 2004; Doyle et al. 2007a). Thus, end-of-year inventory balances reported on firms' balance sheets are likely to be reliable for financial reporting. In contrast, to the extent that inventory reports are generated from the same system that generates financial reports, errors in internal management reports, which are not corrected by auditors, will lead to errors in inventory management. As previously noted, automated inventory orders will affect inventory quantities and costs throughout the year, leading to suboptimal inventory management.

Taken together, although effective ICFR is intended to provide assurance about the reliability of a firm's financial statements, not about its operating performance (Deloitte & Touche et al. 2004), our general hypothesis is that the implications of ineffective ICFR go beyond financial reporting quality. Specifically, we posit that ineffective inventory-related ICFR is associated with poor inventory management. It is important to concretely document that the most fundamental aspects of firm operations, such as inventory management, are affected by ICFR. To the extent that managers and regulators are not aware that effective inventory-related ICFR enables management to make better inventory management decisions, the perceived benefits of effective ICFR will be underestimated. The disconnect between perceived and actual benefits of maintaining effective ICFR potentially influences the ongoing deliberations to exempt even more firms from the auditor assessment of ICFR.

III. SAMPLE AND DESCRIPTIVE STATISTICS

Sample and Data

Our initial sample is comprised of 15,485 accelerated filer firm-years with available data on internal control effectiveness from 2004 to 2009 from the WRDS-based Audit Analytics database. Audit Analytics includes both the evaluation of ICFR as effective or ineffective, as well as the underlying reason(s) for any material weaknesses in internal control, where, by definition, the existence of at least one material weakness in internal control implies ineffective ICFR. Our analysis period begins in 2004 and focuses on accelerated filers because this group of firms has been subject to both Section 404(a) and 404(b) since 2004, whereas non-accelerated filers are only subject to Section 404(a), and only since 2007. We end the analysis period in 2009 in order to have

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[9] The safeguarding of assets is a key objective of internal control (COSO 1992). To the extent that a firm does not restrict access to inventory warehouses or protect inventory from misappropriation, inventory shrinkage will be higher. Normal inventory shrinkage will increase cost of goods sold and decrease ending inventory values, thereby increasing inventory turnover. Thus, the problems of inventory shrinkage biases against finding lower inventory turnover for firms with MWIC related to inventory.

TABLE 1
Sample Selection

Panel A: Sample Selection

	Obs.
Firm-year observations for accelerated filers from Audit Analytics and Compustat for fiscal years 2004–2009	15,485
Less:	
Firm-year observations without inventory information	4,733
Firm-years missing necessary financial data from Compustat to conduct empirical tests	1,799
Sample for inventory turnover tests	8,953
Firm-years with effective internal control over financial reporting	8,313
Firm-years with material weaknesses in internal control over financial reporting (MWIC) other than inventory, but no inventory-related MWIC	479
Firm-years with inventory-related MWIC over financial reporting	161

Panel B: Additional Detail

	2004	2005	2006	2007	2008	2009	Total
Inventory-related MWIC in Audit Analytics	70	63	40	53	34	15	275
Inventory-related MWIC with Compustat data	42	45	29	38	23	10	187
Less MWIC that are removed after manual screen	−5	−8	−4	−2	−3	−4	−26
Inventory-related MWIC used in our tests	37	37	25	36	20	6	161
Remediated inventory-related MWIC	26	31	17	28	18	4	124
Unremediated inventory-related MWIC in year $t+1$	11	6	8	8	2	2	37
Any unremediated MWIC in year $t+1$	27	33	34	18	14	22	148

the same sample for the levels and changes analyses, where the change analyses require two years of future data: the year of remediation and the full year following the remediation.

From the initial sample, we eliminate firm-years missing inventory values (n = 4,733) and the necessary financial data from Compustat to conduct our empirical tests (n = 1,799), resulting in a sample of 8,953 firm-year observations. With the exception of banks, the industry representation is similar to the Compustat population. Untabulated results show that the retail and computer industries are the most represented in our sample, whereas Compustat has the highest representation in banks, then retail and computers, respectively. Of the 8,953 observations, 8,313 firm-year observations relate to firms with effective ICFR. Of the 7.15 percent (640 firm-years) reporting ineffective ICFR, 25.16 percent (161 firm-years) disclosed inventory-related MWIC. We summarize the sample selection process in Table 1, Panel A.

Table 1, Panel B displays the composition of the inventory-related MWIC subsample over time, after reading firms' audit opinions and deleting observations coded in Audit Analytics as having inventory-related MWIC that actually relate to other accounts. Specifically, we exclude 26 observations that were identified as "Inventory, vendor and cost of sales issues" by Audit Analytics, but that we concluded did not actually reflect inventory-related MWIC. In particular, 16 of these directly followed from the inappropriate timing of revenue recognition and related to the corresponding cost of goods sold entry; five related to "vendors," but not inventory; two related to financial statement classification issues, such as current versus non-current inventory; and three appear to be Audit Analytics coding errors.[10] Table 1, Panel B also displays the number of firms that remediated or reported inventory-related MWIC by year.

Descriptive Statistics

Table 2 shows that the inventory turnover ratios of *Inventory-MWIC* firms (mean = 8.370; median = 4.227) are significantly lower than firms with effective ICFR (mean = 14.032; median = 5.955) and *Other-MWIC* firms (mean = 14.336; median = 5.301). This difference is economically significant, with firms reporting inventory-related MWIC holding inventory, on average, for over 40 days versus less than 27 days for the other firms.

Turning to the control variables, among *Inventory-MWIC* firms, revenue recognition internal control problems are present in over 44 percent of the observations, and *Inventory-MWIC* firms tend to have one other MWIC. By construction, the "Effective IC" sample has no MWIC, inventory-related or otherwise. Firms with inventory-related MWIC report lower gross profit and are less capital-intensive relative to firms with effective ICFR and firms with other MWIC. The descriptive statistics indicate that inventory-related MWIC firms have significantly higher sales volatility and sales growth relative to other firms in our sample. Consistent with prior research on ICFR, the descriptive statistics indicate that firms with ineffective ICFR tend to be younger, less likely to employ high-quality auditors, less profitable, and smaller than firms with effective ICFR (Ge and McVay 2005; Ashbaugh-Skaife, Collins, and Kinney 2007; Doyle et al. 2007b).

Table 3 presents Pearson correlations between variables used in our empirical tests. The correlation between *Inventory-MWIC* and *Revenue-MWIC* (correlation = 0.56) highlights how problems with key operating functions can be inter-related, and the correlation between *ROA* and *Loss* (correlation = –0.67) reflects that these are alternative measures of firm performance.

IV. TEST DESIGN AND RESULTS

Inventory Turnover

We begin our empirical analysis by testing the relation between inventory turnover and inventory-related MWIC using the following OLS regression:

$$\begin{aligned} InventoryTurnover_t = {} & \alpha_0 + \alpha_1 Inventory\text{-}MWIC_t + \alpha_2 Revenue\text{-}MWIC_t + \alpha_3 Other\text{-}MWIC_t \\ & + \alpha_4 GrossMargin_t + \alpha_5 CapitalIntensity_{t-1} + \alpha_6 SalesVolatility_{t-1} \\ & + \alpha_7 SalesGrowth_{t-1} + \alpha_8 Segments_{t-1} + \alpha_9 ForeignSales_{t-1} \\ & + \alpha_{10} FirmAge_{t-1} + \alpha_{11} Auditor_{t-1} + \alpha_{12} ROA_{t-1} + \alpha_{13} Loss_{t-1} \\ & + \alpha_{14} Size_{t-1} + IndustryDummies + YearDummies + \varepsilon, \end{aligned} \tag{1}$$

where *InventoryTurnover* is equal to a firm's cost of goods sold in year *t* divided by average inventory over year *t*.[11] We also present an industry-adjusted inventory turnover measure as a

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[10] Table 1, Panel B shows that the number of inventory-related material weaknesses in internal control appearing in the Audit Analytics database declined over our sample period, from 70 (63) in 2004 (2005) to 15 in 2009. Because the economic recession may have impacted inventory turnover and internal controls, in untabulated results, we also replicate our analyses excluding 2007–2009; results are similar.

[11] We adjust all LIFO (last in, first out) inventory figures to be on a FIFO (first in, first out) basis using the required disclosure of LIFO reserves. Our primary analysis defines inventory turnover based on the cost of goods sold. Prior research occasionally defines inventory turnover based on sales to assess the consequences of inventory shortages (Gaur, Fisher, and Raman 2005). As an untabulated robustness check, when we calculate inventory turnover based on sales to capture the effect of inventory shortages due to ineffective ICFR over inventory, results are similar.

TABLE 2

Descriptive Statistics

	Effective IC n = 8,313			*Other-MWIC* n = 479			*Inventory-MWIC* n = 161		
	Mean	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**
InventoryTurnover	14.032	5.955	19.32	14.336	5.301	20.23	8.370***	4.227***	13.45
AverageInventory (in millions)	649.9	114.1	1,634.34	505.9*	59.1***	1,686.26	170.5***	60.3***	300.69
CostofSales (in millions)	4,248.6	777.7	10,776.24	3,349.3*	419.7***	11,690.38	933.1***	253.1***	1,812.70
Revenue-MWIC	0	0	0	0.063***	0.000***	0.24	0.447***	0.000***	0.50
Other-MWIC	0	0	0	1.399***	1.000***	1.18	1.398***	1.000***	1.31
GrossMargin	0.372	0.351	0.22	0.38	0.357	0.24	0.345	0.333	0.18
CapitalIntensity	5.691	5.669	2.23	4.962***	4.947***	2.23	4.414***	4.175***	1.71
SalesVolatility	0.177	0.129	0.15	0.206***	0.165***	0.17	0.232***	0.194***	0.16
SalesGrowth	0.124	0.09	0.29	0.168***	0.1	0.33	0.203***	0.11	0.34
Segment	1.695	1.792	0.75	1.583***	1.609**	0.73	1.642	1.792	0.65
ForeignSales	0.132	0	0.34	0.088***	0.000***	0.28	0.093	0	0.29
FirmAge	2.687	2.708	0.96	2.492***	2.565***	0.97	2.336***	2.485**	1.04
Auditor	0.947	1	0.22	0.930*	1.000*	0.26	0.926*	1	0.26
ROA	0.049	0.055	0.12	0.012***	0.030***	0.14	0.004***	0.024***	0.13
Loss	0.176	0	0.38	0.301***	0.000***	0.46	0.348***	0.000***	0.48
Size	7.374	7.192	1.73	6.718***	6.518***	1.62	6.272***	6.095***	1.19

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*, **, *** Denote two-tailed p-values of less than 0.10, 0.05, and 0.01, respectively, and are testing the difference from the effective internal control sample.

The Effective IC columns relate to firm-year observations with effective internal control over financial reporting. Other-MWIC columns represent the firm-year observations with material weaknesses in internal control other than inventory. Inventory-MWIC columns represent the firm-year observations with inventory-related MWIC. Variable definitions appear in Appendix A. All variables except indicators and *FirmAge* are winsorized at the extreme 1 percent and 99 percent.

TABLE 3

Correlation Matrix

	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1 *InventoryTurnover*	**−0.04**	**−0.02**	**−0.02**	**−0.21**	**0.12**	**−0.03**	**0.03**	**−0.18**	**−0.10**	**−0.11**	0.01	−0.01	**−0.02**	−0.01
2 *Inventory-MWIC*		**0.56**	**0.36**	**−0.02**	**−0.07**	**0.05**	**0.04**	−0.01	−0.01	**−0.05**	−0.01	**−0.05**	**0.06**	**−0.08**
3 *Revenue-MWIC*			**0.37**	0.00	**−0.06**	**0.02**	0.00	0.01	0.00	**−0.02**	0.00	**−0.04**	**0.06**	**−0.07**
4 *Other-MWIC*				0.01	**−0.07**	**0.03**	**0.02**	0.00	−0.01	**−0.04**	**−0.02**	**−0.06**	**0.08**	**−0.08**
5 *GrossMargin*					**−0.11**	**−0.09**	0.10	**0.02**	0.01	**−0.09**	0.00	**0.21**	**−0.08**	**0.11**
6 *CapitalIntensity*						**−0.29**	**−0.12**	**0.23**	0.01	**0.30**	**0.23**	**0.19**	**−0.23**	**0.76**
7 *SalesVolatility*							**0.09**	**−0.14**	**−0.02**	**−0.23**	**−0.07**	**0.04**	**0.06**	**−0.22**
8 *SalesGrowth*								**−0.15**	**−0.17**	**−0.17**	0.01	**0.10**	**−0.09**	0.00
9 *Segments*									**0.27**	**0.20**	**0.07**	**0.07**	**−0.03**	**0.26**
10 *ForeignSales*										**0.05**	**−0.03**	**−0.03**	**0.05**	0.01
11 *FirmAge*											**0.03**	**0.06**	**−0.12**	**0.25**
12 *Auditor*												**0.08**	**−0.08**	**0.24**
13 *ROA*													**−0.67**	**0.27**
14 *Loss*														**−0.29**
15 *Size*														

Statistically significant correlations (p-values < 0.10, two-tailed) are in bold.
Variable definitions appear in Appendix A.

second dependent variable, where *IndAdj-InventoryTurnover* is defined as the firm's inventory turnover less the median inventory turnover for the firm's industry, based on two-digit SIC codes. We expect a negative coefficient on *Inventory-MWIC*, which is an indicator variable identifying firms that disclose inventory-related MWIC.

Our model of inventory turnover includes three sets of control variables. The first set of controls contains indicator variables capturing whether the firm discloses an MWIC related to an area other than inventory. Specifically, we control for MWIC related to revenue recognition (*Revenue-MWIC*) because sales and inventory are related via cost of goods sold, but they are not perfectly aligned because of inventory holdings across accounting periods. We also control for other MWIC (*Other-MWIC*) to reduce the concern that a correlated omitted variable drives both ineffective ICFR and inventory turnover.[12]

The second set of control variables in Equation (1) contains measures that prior research documents influence firms' inventory turnover, namely, *GrossMargin, CapitalIntensity, SalesVolatility,* and *SalesGrowth*. We include contemporaneous gross profit margin (*GrossMargin*) as a control in Equation (1) because the trade-off between gross margin and inventory turnover is strategically determined. Specifically, firms that are able to sustain high gross margins tend to report lower inventory turnover, and *vice versa* (Gaur et al. 2005). The remainder of our variables are measured in year $t-1$. *CapitalIntensity,* defined as the natural logarithm of gross property, plant, and equipment, is included as a determinant of inventory turnover because firms that invest in more fixed assets, such as warehouses, stores, or leasehold improvements, are likely to have higher inventory turnover to generate margins to cover their overhead costs (Balakrishnan, Linsmeier, and Venkatachalam 1996). Firms with more volatile sales (*SalesVolatility*) are expected to have lower inventory turnover because the uncertainty in the demand for products will increase the likelihood of holding costs or stock-outs. We expect firms with greater sales growth (*SalesGrowth*) to have higher inventory turnover because the greater demand for products would result in greater inventory turnover.[13]

The third set of control variables includes other determinants of ineffective ICFR as reported in Ashbaugh-Skaife et al. (2007) and Doyle et al. (2007b). These determinants include firm complexity (*Segments* and *ForeignSales*), firm age (*FirmAge*), audit quality (*Auditor*), limited resources (*ROA* and *Loss*), and firm size (*Size*). Firms with more complex operations are more likely to have ineffective ICFR because the more diverse a firm's operations, the greater the chance there are inconsistent policies and procedures across operating or geographic segments. The natural log of the number of geographic and operating segments (*Segments*) and the presence of foreign sales (*ForeignSales*) are used to proxy for firm complexity. *FirmAge,* proxied by the natural log of the number of years that a company is covered by CRSP, is included in Equation (1) because smaller, younger firms are less likely to invest in sophisticated information systems, thereby increasing the likelihood of having ineffective ICFR. The quality of the external auditor is known to affect the likelihood of detecting and reporting firms' material weaknesses in ICFR. *Auditor* is set equal to 1 if the firm hires one of the six largest U.S. audit firms during our period of analysis (Deloitte, Ernst & Young, KPMG, PricewaterhouseCoopers, BDO Seidman, or Grant Thornton), and 0 otherwise. The effectiveness of ICFR is also a function of resources, as firms with limited resources are more likely to underinvest in systems and controls and to have staffing problems that lead to ineffective ICFR. We include the prior year's return on assets (*ROA*) and the existence of a loss in the prior year (*Loss*) to proxy for firm resources. We include the natural log of market value of assets to proxy for firm size (*Size*) because larger firms are more likely to have effective ICFR. We control for ineffective ICFR determinants to reduce concerns of correlated omitted variables. However, we do not make signed predictions on the set of ICFR control variables, as it is unclear how they are systematically related to inventory turnover. Equation (1) also includes industry and year indicator variables. We cluster standard errors by firm and provide variable definitions in Appendix A.

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[12] *Other-MWIC* would include material weaknesses due to the lack of separation of duties that arguably also affect the efficiency and effectiveness of operations, but are less directly traced to fundamental operating performance measures such as inventory turnover or inventory impairments.

[13] Firms having greater *SalesVolatility* and higher *SalesGrowth* are also more likely to have internal control problems because of the difficultly in planning, implementing, and maintaining adequate policies and procedures over periods of rapid expansion or contraction (Ashbaugh-Skaife et al. 2007; Doyle et al. 2007b).

The results of the inventory turnover analysis are presented in the first column of results in Table 4. We begin by discussing the estimation of Equation (1) using *InventoryTurnover,* the unadjusted measure of inventory turnover. Consistent with our expectations, we find a significantly negative coefficient on *Inventory-MWIC* (coefficient = –6.104; t-statistic = –3.91), indicating inventory-related internal control problems are associated with lower inventory turnover. In contrast, the coefficients on *Revenue-MWIC* (coefficient = 1.322; t-statistic = 0.59) and *Other-MWIC* (coefficient = 0.291; t-statistic = 0.62) are both insignificant, indicating that material weaknesses in ICFR that are not specifically related to inventory are not associated with inventory turnover. This result is important because finding no association between other types of material weaknesses and inventory turnover alleviates the natural concern that a correlated omitted variable, such as poor management, could drive both the inventory-related ineffective ICFR and the low turnover ratio.

Turning to the factors posited to be related to inventory turnover, we find in Table 4 that *GrossMargin* is negatively associated with *InventoryTurnover,* consistent with the strategic association of high margins and low inventory turnover, and *vice versa* (Nissim and Penman 2001; Gaur et al. 2005). We find a significantly positive coefficient on *CapitalIntensity,* supporting the notion that inventory turnover is higher in firms that face higher fixed costs. We find a significantly negative coefficient on *SalesVolatility,* suggesting that the uncertainty in the demand for products results in managers being less able to predict efficient inventory levels. Finally, as predicted, we find a significantly positive coefficient on *SalesGrowth,* as more sales drives higher inventory turnover.

Turning to the MWIC determinants included in Equation (1), for which we did not have sign predictions, we find significantly negative coefficients on *Segments, ForeignSales,* and *FirmAge* in Table 4, indicating that firms with more diverse operations, competing in foreign product markets, and older firms have lower inventory turnovers. We find a positive association between turnover and *ROA,* which is consistent with a higher turnover increasing *ROA* after controlling for gross margin. Finally, *Size* is negatively associated with turnover.

The second column in Table 4 reports the results using *IndAdj-InventoryTurnover* as the dependent variable. The sign and significance level on *Inventory-MWIC* are quantitatively and qualitatively similar to the original analysis (coefficient = –8.218; t-statistic = –3.25). Likewise, the coefficients on the other MWIC variables (*Revenue-MWIC* and *Other-MWIC*) are insignificant, similar to the analysis using the unadjusted inventory turnover measure as the dependent variable. With the exception of the insignificant coefficient on *SalesVolatility* and the positive and marginally significant coefficient on *Size,* the results on the control variables are similar to those of the unadjusted inventory turnover analysis.[14]

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[14] Gaur et al. (2005) suggest that inventory turnover is better evaluated on a quarterly basis because of seasonality in inventory supply and demand. Although SOX Section 302 disclosures are less accurate in capturing the effectiveness of ICFR and quarterly ending inventory balances are not audited, we reestimate Equation (1) using quarterly data and inventory-related internal control deficiencies disclosed under Section 302. Untabulated results show that inventory-related SOX Section 302 disclosures are negatively associated with inventory turnover (p-value = 0.017) and are marginally negatively associated with industry-adjusted inventory turnover (p-value = 0.110).

TABLE 4

Regression Analysis of Inventory Turnover Ratio

		Dependent Variable =			
	Pred. Sign	***Inventory Turnover***	***IndAdj-Inventory Turnover***	***Inventory Turnover***	***IndAdj-Inventory Turnover***
Intercept		32.659***	26.169***	32.672***	26.185***
		(12.80)	(4.36)	(12.80)	(4.36)
Inventory-MWIC	–	−6.014***	−8.218***		
		(−3.91)	(−3.25)		
Inventory-Tracking-MWIC	–			−4.308***	−7.354***
				(−2.32)	(−2.70)
Inventory-Valuation-MWIC	–			−4.104***	−3.439*
				(−2.67)	(−1.50)
Revenue-MWIC		1.322	−1.596	1.082	−2.218
		(0.59)	(−0.42)	(0.51)	(−0.62)
Other-MWIC		0.291	−0.212	0.291	−0.242
		(0.62)	(−0.18)	(0.61)	(−0.20)
GrossMargin	–	−18.332***	−38.271***	−18.338***	−38.285***
		(−9.33)	(−6.45)	(−9.33)	(−6.46)
CapitalIntensity	+	1.781***	1.713**	1.779***	1.716**
		(5.74)	(2.30)	(5.73)	(2.31)
SalesVolatility	–	−6.391**	−6.367	−6.413**	−6.444
		(−2.17)	(−0.83)	(−2.17)	(−0.84)
SalesGrowth	+	3.087***	4.612**	3.075***	4.581**
		(3.16)	(2.07)	(3.15)	(2.06)
Segments		−4.468***	−3.902***	−4.472***	−3.911***
		(−7.49)	(−2.72)	(−7.50)	(−2.73)
ForeignSales		−4.367***	−3.035**	−4.353***	−3.019**
		(−7.66)	(−2.49)	(−7.63)	(−2.47)
FirmAge		−2.502***	−3.493***	−2.505***	−3.501***
		(−5.93)	(−3.90)	(−5.94)	(−3.91)
Auditor		0.287	1.283	0.258	1.255
		(0.22)	(0.43)	(0.20)	(0.42)
ROA		5.909*	14.670**	5.918*	14.709**
		(1.88)	(2.17)	(1.88)	(2.17)
Loss		−0.605	1.668	−0.597	1.690
		(−0.73)	(1.01)	(−0.72)	(1.03)
Size		−1.003***	1.599*	−0.997***	1.609*
		(−2.67)	(1.83)	(−2.66)	(1.84)
Year Indicators		Included	Included	Included	Included
Industry Indicators		Included	Not Included	Included	Not Included
Firm-Year Observations		8,953	8,953	8,953	8,953
Inventory-MWIC		161	161		
Inventory-Tracking MWIC				99	99
Inventory-Valuation MWIC				115	115
F-statistic		48.67	20.59	47.35	20.06
Adjusted R^2		0.161	0.073	0.161	0.073

(continued on next page)

TABLE 4 (continued)

*, **, *** Denote p-values of less than 0.10, 0.05, and 0.01, respectively (one-tailed if there is a sign prediction, two-tailed otherwise).

Standard errors are clustered by firm, and t-statistics are in parentheses. Note that the coefficients on *Inventory-Tracking-MWIC* and *Inventory-Valuation-MWIC* are not statistically different under an F-test at conventional levels.

Variable definitions appear in Appendix A.

Type of Inventory Weakness Analysis

We next explore whether the relation between inventory turnover and inventory-related MWIC is driven by inventory tracking, inventory valuation, or both types of inventory-related weaknesses. For example, consider the disclosure made by 99 Cents Only Stores (excerpted from their 2006 10-K) in its internal control report:

> There was an internal control weakness surrounding the Company's inventory accounts. The Company *did not maintain accurate records of specific item quantity and location of its inventory* and therefore relied primarily on physical counting of inventory and its existing transactional controls. (emphasis added)

Similarly, Dana Holding Corp's (excerpted from their 2004 10-K) internal control report states:

> The Company *did not maintain effective control over the valuation of certain inventory and the related cost of goods sold accounts*. Specifically, the Company did not maintain effective controls over the computation and review of its LIFO inventory calculation to ensure that appropriate components, such as the impact of steel surcharges, were properly reflected in the calculation. (emphasis added)

Although we expect both tracking and valuation issues to affect inventory management, tracking issues are likely to have a larger economic effect. If inventory quantity is mistakenly under- (over-) reported because inventory is not tracked properly, then too many (too few) goods will be ordered, resulting in an overstock (stock-out) and additional holding (stock-out) costs (Nachtmann et al. 2010).

We read each of the 161 adverse inventory-related MWIC disclosures to classify each as "tracking" or "valuation," and reestimate Equation (1).[15] The final two columns in Table 4 display the results. We find lower inventory turnover ratios for both types of inventory-related MWIC; for example, in the third column of results, the coefficient on inventory-tracking (valuation) is −4.308 (−4.104). Untabulated results show that the coefficients on *Inventory-Tracking-MWIC* and *Inventory-Valuation-MWIC* are not statistically different under an F-test in either specification. The signs and significance levels of the coefficients on the control variables are quantitatively the same as in our main analysis reported in the first two columns of Table 4.

Remediation Analysis

If the lower inventory turnover is a result of inventory-related MWIC, then remediation of these problems should produce an increase in inventory turnover. As an illustration of remediation of inventory-related MWIC, JDS Uniphase Corporation disclosed the following in their 10-K for the year ended June 30, 2007:[16]

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[15] Note that 53 of the observations have both tracking and valuation issues. We also note that 37 of the 161 firm-years with inventory-related MWIC also recognized an information technology (IT)-related MWIC, with 20 of these affecting inventory to some extent. In untabulated analyses, we insert indicator variables for IT-related MWIC and co-mingled inventory-IT MWIC, but neither variable is significant at conventional levels.

> As of July 1, 2006, the Company determined that it did not maintain effective controls . . . over the accounting for the completeness, existence, accuracy and valuation of inventory and cost of goods sold. Specifically, adequate controls were not designed over (1) the existence and accuracy of the perpetual inventory balance, (2) the accuracy of the standard costs and analysis of variances (3) the valuation of excess and obsolete inventory. In addressing this material weakness, the Company implemented the following changes to our internal control over financial reporting:
>
> - Implementation of Oracle for CommTest U.S. locations
> - Adoption of JDSU Corporate ("Classic") controls and procedures in CommTest U.S.
> - Addition of Manufacturing Site Controller and multiple cost accounting analysts
>
> As of June 30, 2007, we have determined that the new controls are effectively designed and have demonstrated effective operation for a sufficient period of time to enable management to conclude that this material weakness has been remediated.

To investigate whether firms that remediate their inventory-related MWIC experience an increase in their inventory turnover, we estimate the following change regression:

$$\begin{aligned}\Delta TurnoverRatio_{t,t+2} = {} & \alpha_0 + \alpha_1 Remediation\text{-}Inventory\text{-}MWIC_{t,t+1} \\ & + \alpha_2 Remediation\text{-}Revenue\text{-}MWIC_{t,t+1} \\ & + \alpha_3 Remediation\text{-}Other\text{-}MWIC_{t,t+1} + \Delta Controls + \varepsilon. \end{aligned} \quad (2)$$

The sample used in our remediation analysis is comprised of 124 firms in the original sample that remediated their inventory-related MWIC in year $t+1$ (see Table 1, Panel B), compared to 148 firms that reported any MWIC in year t, and continue to report any MWIC in year $t+1$. This results in a total of 272 observations; of the 124 firms that remediated their inventory-related MWIC, 85 also remediated their revenue-related or other MWIC, which we control for in the remediation model.[17]

Remediation-Inventory-MWIC is equal to 1 if the problem was remediated, as evidenced by there being an inventory-related material weakness in year t, but not year $t+1$, and 0 otherwise. We expect a positive coefficient on *Remediation-Inventory-MWIC* if the implementation of effective policies and procedures over inventory acquisition, tracking, and valuation improve inventory management. *Remediation-Revenue-MWIC* is equal to 1 if there was a revenue recognition-related material weakness in year t, but not year $t+1$, and 0 otherwise. *Remediation-Other-MWIC* is equal to 1 if there were other MWIC in year t, but not year $t+1$, and 0 otherwise. We include other types of remediation in the model to assess whether remediating other material weaknesses in internal control over financial reporting is associated with the change in inventory turnover. Finding insignificant coefficients on *Remediation-Revenue-MWIC* and *Remediation-Other-MWIC* would provide further support for the effect of the remediation of inventory MWIC on the change in inventory turnover.

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[16] Firms often make more general statements regarding the remediation of inventory-related MWICs. For example, in its 2008 10-K, Navistar International Corp. stated that "Management will implement stronger inventory procedures and systems and augment our resources to address the inventory accounting material weakness. Specifically, we are changing our procedures for cost accounting, conducting physical inventory counts, establishing accruals for inventory receipts, and establishing our provisions for inventory obsolescence." Despite the lack of greater specificity in such disclosures, the lack of an inventory-related MWIC subsequent to having an inventory-related MWIC is consistent with the firm implementing effective inventory tracking and valuation systems.

[17] We do not compare the 124 inventory-MWIC remediators to the firms that continue to report inventory-related MWIC because of the small sample of firms that continue to report inventory-related MWIC ($n = 37$; see Table 1, Panel B). In untabulated analyses, the difference in the change in inventory turnover between the two subsamples is 0.481, but this difference is not statistically different from zero (t-statistic = 1.22).

We measure inventory turnover changes from year *t* to year *t*+2, where year *t*+1 is the remediation year. We do not examine one-year changes because it is not clear when in year *t*+1 the inventory-related material weakness was remediated.[18] Thus, we require the weakness to have been remediated for *at least* one full year (all of year *t*+2) in order to better capture the benefits to remediation. As such, all control variables are also measured as two-year changes defined as (*t*+2) – *t*.

The first column of results in Table 5 displays the results of estimating Equation (2). We find an insignificant coefficient on *Remediation-Inventory-MWIC* (coefficient = 1.262; t-statistic = 0.48) and no significant relation between remediation of other types of internal control problems and changes in inventory turnover. However, when we partition remediating firms by the nature of their inventory-related MWIC, presented in the final column of results in Table 5, we discover that firms that remediate their inventory-tracking MWIC experience significant increases in their inventory turnover rates (coefficient = 4.253; t-statistic = 1.65). Because inventory tracking relates to having the necessary policies and procedures to properly record the buying, selling, storage, and returns of inventory, this finding is consistent with the notion that the day-to-day inventory management decisions are affected by MWIC related to inventory tracking. Importantly, the coefficients on *Remediation-Revenue-MWIC* and *Remediation-Other-MWIC* are insignificant in both regressions, diminishing the concern that the remediation of other types of MWIC drives the change in inventory turnover. The results of the remediation analysis also indicate that as gross margin increases, inventory turnover falls, as theory predicts.

Inventory Impairments

As stated earlier, surplus inventory can be an indication of inventory management problems. Firms holding surplus or obsolete inventory with market values declining below cost are required to record inventory impairments in order to value the inventory at the lower of cost or market in accordance with U.S. GAAP. According to U.S. GAAP, inventory impairments must be written off in the period in which they are identified. The write-off is recorded as a loss and reduces operating income, thereby having a negative effect on operating performance and bottom-line net income. We use both the existence and the magnitude of inventory impairments (*Impairment Indicator* and *Impairment Magnitude,* respectively) as indications of poor inventory management and deterioration of operating performance as a result of inventory-related MWIC.

Because the presence and magnitude of inventory impairments is hand-collected from firms' financial statements, we construct a matched sample, based on year, two-digit SIC code, total assets, and inventory level, to investigate our conjecture that inventory-related MWIC will increase the likelihood of an inventory impairment, as well as the magnitude of inventory impairments. Of the 161 inventory-related MWIC firm-years we examine in Table 4, we are able to match 139 with control firms that report effective internal control.[19] In untabulated analyses, we confirm that total

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[18] Although we require that the MWIC related to inventory be remediated by the end of year *t*+1, the day-to-day operations throughout year *t*+1 could still be affected by the inventory-related internal control problems, especially given the time and effort involved in implementing inventory control systems (Bedard, R. Hoitash, U. Hoitash, and Westermann 2012).

[19] Specifically, within year and two-digit SIC code, we first consider all match firms with total assets within 30 percent of the material weakness firm, and then identify the matched firm as the one with the closest inventory level. It is important to note that many of the control variables in the inventory impairment models are also determinants of MWIC, thereby diminishing endogeneity concerns and the need for a propensity matched sample. Untabulated results, however, are robust to estimating a propensity score match using the following model: $Inventory\text{-}MWIC = \beta_0 + \beta_1 Size + \beta_2 CapitalIntensity + \beta_3 SalesVolatility + \beta_4 SalesGrowth + \beta_5 ROA + \beta_6 Loss + \beta_7 Segments + \beta_8 ForeignSales + \beta_9 FirmAge + \beta_{10} Auditor + Year\ Fixed\ Effects + Firm\ Fixed\ Effects$.

TABLE 5

Regression Analysis of Changes in Inventory Turnover Ratio on Remediation of Inventory-Related Material Weaknesses

	Pred. Sign	Dependent Variable = Δ*InventoryTurnover*	
Intercept		0.769	0.266
		(0.33)	(0.11)
Remediation-Inventory-MWIC	+	1.262	
		(0.48)	
Remediation-Inventory-Tracking	+		4.253**
			(1.65)
Remediation-Inventory-Valuation	+		−0.382
			(−0.15)
Remediation-Revenue-MWIC		1.157	0.715
		(0.43)	(0.27)
Remediation-Other-MWIC		3.937	3.309
		(1.38)	(1.19)
Δ*GrossMargin*		−10.653**	−10.892**
		(−2.05)	(−2.11)
Δ*CapitalIntensity*		−2.581	−2.750
		(−1.43)	(−1.52)
Δ*SalesVolatility*		4.653	4.574
		(0.46)	(0.45)
Δ*SalesGrowth*		1.110	1.261
		(0.43)	(0.95)
Δ*Segment*		1.798	2.170
		(0.79)	(0.95)
Δ*ForeignSales*		−3.122	−2.669
		(−1.17)	(−1.00)
Δ*Auditor*		−5.799*	−6.016*
		(−1.78)	(−1.84)
Δ*ROA*		4.305	4.371
		(0.45)	(0.46)
Δ*Loss*		−0.185	0.079
		(−0.09)	(0.04)
Δ*Size*		−0.199	−0.201
		(−0.14)	(−0.14)
PriorPeriodInventoryTurnover		−0.126**	−0.122**
		(−2.11)	(−2.04)
Year Indicators		Included	Included
Inventory-Related MWIC Remediators		124	124
All MWIC Non-Remediators		148	148
Total Firm-Year Observations		272	272
F-statistic		1.54	1.60
Adjusted R^2		0.04	0.04

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*, ** Denote p-values of less than 0.10 and 0.05, respectively (one-tailed if there is a sign prediction, two-tailed otherwise).

(continued on next page)

TABLE 5 (continued)

The sample used in this analysis is comprised of 124 firms in the original sample described in Table 1 that remediated their inventory-related MWIC in year $t+1$, compared to 148 firms that had any MWIC in year t and did not remediate their MWIC in year $t+1$; both must have the necessary data to calculate change variables. The change variables are defined as $(t+2) - t$, where t is the last year the firm reported an MWIC. Variables are winsorized at the 1 percent level. Standard errors are clustered by firm, and t-statistics are in parentheses.
Variable definitions not shown below appear in Appendix A.

Variable Definitions:
Remediation-Inventory-MWIC = 1 if the firm corrected its inventory-related internal control problem, and 0 otherwise;
Remediation-Revenue-MWIC = 1 if the firm corrected its revenue recognition internal control problem, and 0 otherwise;
Remediation-Other-MWIC = 1 if the firm corrected its other internal control problems, and 0 otherwise; and
PriorPeriodInventoryTurnover = the inventory turnover from year $t-1$.

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assets, inventory, and cost of goods sold are not statistically different across the treatment and control groups.

Untabulated descriptive statistics indicate that 43.8 percent of *Inventory-MWIC* firms report inventory impairments, whereas 22.3 percent of control firms incur inventory write-offs, and the difference is significant (p-value = 0.001). To test the association between inventory-related internal control problems and inventory impairments (i.e., write-offs) in a multivariate setting, we estimate the following equations:

$$\begin{aligned} \mathit{Impairment\ Indicator}_t = {} & \alpha_0 + \alpha_1 \mathit{Inventory\text{-}MWIC}_t + \alpha_2 \mathit{Revenue\text{-}MWIC}_t + \alpha_3 \mathit{Other\text{-}MWIC}_t \\ & + \alpha_4 \mathit{GrossMargin}_t + \alpha_5 \mathit{CapitalIntensity}_{t-1} + \alpha_6 \mathit{SalesVolatility}_{t-1} \\ & + \alpha_7 \mathit{SalesGrowth}_{t-1} + \alpha_8 \mathit{Segments}_{t-1} + \alpha_9 \mathit{ForeignSales}_{t-1} \\ & + \alpha_{10} \mathit{FirmAge}_{t-1} + \alpha_{11} \mathit{Auditor}_{t-1} + \alpha_{12} \mathit{ROA}_{t-1} + \alpha_{13} \mathit{Loss}_{t-1} \\ & + \alpha_{14} \mathit{Size}_{t-1} + \varepsilon. \end{aligned} \tag{3}$$

$$\begin{aligned} \mathit{Impairment\ Magnitude}_t = {} & \alpha_0 + \alpha_1 \mathit{Inventory\text{-}MWIC}_t + \alpha_2 \mathit{Revenue\text{-}MWIC}_t + \alpha_3 \mathit{Other\text{-}MWIC}_t \\ & + \alpha_4 \mathit{GrossMargin}_t + \alpha_5 \mathit{CapitalIntensity}_{t-1} + \alpha_6 \mathit{SalesVolatility}_{t-1} \\ & + \alpha_7 \mathit{SalesGrowth}_{t-1} + \alpha_8 \mathit{Segments}_{t-1} + \alpha_9 \mathit{ForeignSales}_{t-1} \\ & + \alpha_{10} \mathit{FirmAge}_{t-1} + \alpha_{11} \mathit{Auditor}_{t-1} + \alpha_{12} \mathit{ROA}_{t-1} + \alpha_{13} \mathit{Loss}_{t-1} \\ & + \alpha_{14} \mathit{Size}_{t-1} + \varepsilon. \end{aligned} \tag{4}$$

We expect positive coefficients on *Inventory-MWIC*. We estimate Equation (3) using a logistic regression and estimate Equation (4) using a tobit regression because impairment magnitude cannot be less than zero and, thus, is left censored. We do not include year or industry indicators in Equations (3) or (4) because, as previously discussed, we use a matched sample in conducting our inventory impairment analysis. Otherwise, Equations (3) and (4) include the same control variables as in Equation (1) and standard errors are clustered by firm.

The first two columns of results in Table 6 display the results of our inventory impairment tests, with the likelihood of inventory impairment analysis (Equation (3)) being reported in the first column, labeled "*Impairment,*" and the magnitude of impairment analysis (Equation (4)) in the second column. Consistent with our expectations, we find firms with inventory-related MWIC are more likely to record inventory impairments (coefficient = 0.717; $\chi^2 = 3.45$). The untabulated economic effect of *Inventory-MWIC* suggests that having an inventory-related MWIC increases the likelihood of writing off inventory by 15.9 percent. The results also indicate that firms with higher sales growth and larger auditors are more likely to report

TABLE 6

Logistic (Tobit) Regression Analysis of the Existence (Magnitude) of Inventory Impairments on MWIC

		Dependent Variable =			
	Pred. Sign	*Impairment*	*Impairment Magnitude*	*Impairment*	*Impairment Magnitude*
Intercept		−3.569***	−0.033**	−3.547***	−0.032**
		(9.14)	(5.78)	(9.19)	(5.02)
Inventory-MWIC	+	0.717**	0.011***		
		(3.45)	(5.90)		
Inventory-Tracking-MWIC				0.442*	0.005*
				(1.75)	(1.70)
Inventory-Valuation-MWIC				0.568**	0.007**
				(2.67)	(3.22)
Revenue-MWIC		0.200	0.001	0.213	0.002
		(0.27)	(0.04)	(0.31)	(0.14)
Other-MWIC		0.104	0.001	0.089	0.002
		(0.46)	(0.53)	(0.33)	(0.67)
GrossMargin		−0.301	−0.004	−0.271	−0.004
		(0.15)	(0.19)	(0.12)	(0.15)
CapitalIntensity		−0.083	−0.003**	−0.069	−0.003**
		(0.44)	(4.87)	(0.31)	(4.34)
SalesVolatility		1.068	0.006	1.072	0.006
		(1.40)	(0.28)	(1.41)	(0.28)
SalesGrowth		1.057**	0.008	1.106**	0.009
		(4.79)	(2.16)	(5.20)	(2.48)
Segments		0.281	0.004	0.297	0.004
		(1.63)	(1.80)	(1.79)	(1.99)
ForeignSales		0.335	0.001	0.280	0.001
		(0.46)	(0.01)	(0.31)	(0.01)
FirmAge		−0.183	−0.003	−0.189	−0.003
		(1.39)	(1.86)	(1.47)	(1.90)
Auditor		1.395*	0.004	1.444**	0.005
		(3.75)	(0.35)	(4.00)	(0.50)
ROA		−0.582	0.005	−0.651	0.004
		(0.12)	(0.08)	(0.15)	(0.03)
Loss		0.203	0.003	0.168	0.003
		(0.23)	(0.46)	(0.16)	(0.38)
Size		0.206	0.004*	0.186	0.004*
		(1.54)	(3.56)	(1.28)	(3.25)
Year Indicators		Included	Included	Included	Included
Firm-Year Observations		278	278	278	278
Inventory-Tracking-MWIC				86	86
Inventory-Valuation-MWIC				99	99
χ^2		39.21	277.02	39.90	275.45
Pseudo R^2		0.183	NA	0.186	NA

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*, **, *** Denote p-values of less than 0.10, 0.05, and 0.01, respectively (one-tailed if there is a sign prediction, two-tailed otherwise).

(continued on next page)

TABLE 6 (continued)

The sample used in this analysis is comprised of 139 unique firms described in Table 1 that reported inventory-related material weaknesses in internal control that could be matched to 139 firms with effective internal control based on two-digit SIC code, year, total assets, and inventory value. Standard errors are clustered by firm, and χ^2 statistics are in parentheses. Note that the coefficients on *Inventory-Tracking-MWIC* and *Inventory-Valuation-MWIC* are not statistically different under an F-test at conventional levels.
Variable definitions appear in Appendix A.

inventory impairments. Other types of MWIC are not associated with the existence of inventory impairments.

Turning to the impairment magnitudes in Table 6, we find a positive and significant coefficient on *Inventory-MWIC* (coefficient = 0.011; $\chi^2 = 5.90$), indicating that firms with ineffective ICFR over inventory report larger inventory impairments relative to the control sample of effective ICFR firms. The results also indicate that firms with higher capital intensity report lower magnitudes of inventory impairments, whereas larger firms tend to report larger inventory impairments. Neither the *Revenue-MWIC* nor the *Other-MWIC* coefficient is significant.

The final two columns of Table 6 display the results of estimating Equations (3) and (4) when partitioning *Inventory-MWIC* into *Inventory-Tracking-MWIC* and *Inventory-Valuation-MWIC*. Regardless of the type of MWIC over inventory, we find firms are more likely to have inventory impairments and report larger inventory impairments when they have ineffective ICFR related to inventory. In untabulated tests, the differences in the coefficients between *Inventory-Tracking-MWIC* and *Inventory-Valuation-MWIC* are not statistically different in either analysis.

Collectively, the results of the inventory turnover analysis and inventory impairment tests are consistent with our general hypothesis that ineffective ICFR related to inventory has implications for firm operations and, ultimately, operating performance.

Change in Additional Operating Performance Measures

Previously, we provided some evidence that firms that remediated their inventory-related MWIC saw improvements in their core operations via greater inventory turns (see Table 5), but this remediation analysis does not shed light on whether the improvements in inventory-related ICFR affect other measures of operating performance. If the effect of the remediation of inventory-related MWIC is substantial, then we expect sales, gross margin, operating income, and cash flows from operations to improve after their remediation. We investigate whether there is a significant increase in sales and gross margin after firms remediate their inventory-related MWIC because these two accounting measures are directly impacted by inventory management and are often reported in the financial press. Likewise, we examine whether there is a significant increase in operating income and operating cash flows because these measures are often cited when discussing firms' operating performance.

We test whether remediating firms' changes in sales, gross margin, operating income, and operating cash flows are greater than zero, and also test whether the changes for firms remediating inventory-related MWIC (124 firm-year observations) are greater than the changes for firms that continue to report any type of MWIC (148 firm-year observations). Table 7, Panel A reports the results. As in Table 5, we calculate changes from year *t* to year *t*+2, where year *t*+1 is the year of remediation.

In Table 7, Panel A, we find that firm-specific changes in sales, gross margin, and cash flow from operations are statistically greater than zero after firms remediate their inventory-related MWIC, suggesting that the remediation of inventory-related MWIC improved firms' operating decisions. For example, among the 124 firm-years remediating their inventory-related MWIC, there

TABLE 7

Operational Performance after Remediation of Inventory-Related Material Weaknesses

Panel A: The Comparison of Changes in Performance between Firms that Remediated Inventory-Related Material Weaknesses and Firms that Did Not Remediate Material Weaknesses

	Firms that Remediated Their Inventory-Related MWIC n = 124	Firms that Did Not Remediate Their MWIC (Any Area) n = 148	
	Mean		t-stat.
Change in log of sales	0.076**	0.146	−1.25
Change in gross profit/net sales	0.021***	−0.007	1.46
Change in operating income/sales	0.026	−0.110	1.37
Change in cash flows from operations/assets	0.027***	−0.009	2.11

, * Denote two-tailed p-values of less than 0.05 and 0.01, respectively, testing for a value greater than 0.

The sample used in this analysis is comprised of 124 firms in the original sample described in Table 1 that remediate their inventory-related MWIC in year $t+1$ compared to 148 firms that did not remediate their MWIC in year $t+1$; both must have the necessary data to calculate the performance measures pre- and post-remediation. The change variables are defined as $(t+2) - t$, where t is the last year the firm reported an MWIC. Variables are winsorized at the 1 percent level.

Panel B: The Comparison of Post-Remediation Performance between Firms that Remediated Inventory-Related Material Weaknesses and Firms that Did Not Have Material Weaknesses

	Firms that Remediated Their Inventory-Related MWIC n = 124	Firms that Consistently Maintained Effective Internal Control n = 7,945	
	Mean		t-stat.
Inventory turnover	10.688	14.230	−1.59
Log of sales	6.272	7.365	−8.41
Gross profit/net sales	0.359	0.371	−0.66
Operating income/sales	−0.004	−0.065	1.07
Cash flows from operations/assets	0.078	0.095	−1.59

The sample used in this analysis is comprised of 124 firms in the original sample described in Table 1 that remediated their inventory-related MWIC in year $t+1$ compared to 7,945 firm-years in the original sample that disclosed effective internal controls in year t and $t+1$; both groups must have the necessary data to calculate the performance measures. The variables are measured at $t+2$, where t is the last year the firm reported an MWIC. Variables are winsorized at the 1 percent level.

is an increase in gross margin of 2.1 percent, on average, from year t to year $t+2$. We also provide evidence that the increase in operating cash flows is significantly greater for remediating firms (0.027) relative to firms that continue to report MWIC (−0.009). Overall, the firm-specific improvements in these performance measures, as well as the improvement in inventory turnover, is consistent with our conjecture that inventory-related MWIC has adverse consequences for firm operations.

Table 7, Panel B reports the results of investigating whether firms that remediated their inventory-related MWIC (124 firm-year observations) are distinguishable from firms that consistently maintained effective ICFR (7,945 firm-year observations). Once firms with inventory-related MWIC remediate these weaknesses, we do not expect the performance of these firms to be different from firms with effective internal control. We find that inventory turnover ratios are lower among the newly remediated firms (10.688) relative to effective ICFR firms (14.230), but the difference is not significant at conventional levels (t-statistic = 1.59; two-tailed p-value = 0.115). Sales are significantly higher (t-statistic = 8.41) among effective ICFR firms (7.365) relative to remediated firms (6.272). In contrast, gross margin and operating margin are not statistically different between firms that remediate their inventory-related MWIC and firms with consistently effective ICFR. Finally, operating cash flows of remediated firms (0.078) are lower than firms consistently maintaining effective internal control (0.095), but the difference is not significant at conventional levels (t-statistic = 1.59; two-tailed p-value = 0.112). Overall, the results of our operational performance analysis provide some evidence that firms opting to remediate their inventory-related MWIC see improvements in their operating performance and become more comparable, in terms of operating performance measures, to firms that have maintained effective ICFR.

Generalizability of the Results

A strength of our research design is the clear link between inventory-related MWIC and inventory management outcome variables: inventory turnover and inventory impairments. However, we expect that our results have broader implications for firm operations. In particular, we expect MWIC to have a negative impact on firm operations in a multitude of ways. For example, ineffective ICFR over accounts receivable can result in a company failing to follow up on outstanding receivables, leading to a greater proportion of uncollectible accounts or failing to recognize the riskiness of potential customers, resulting in higher bad debt expense and lower net income.[20] Similarly, ineffective ICFR over sales-related liabilities such as warranties can lead to inefficient pricing. To provide evidence on this broader association between ineffective ICFR and operations, we examine the relation between *MWIC* and return on assets (*ROA*). The results are reported in Table 8.

We consider three sets of analyses. In the first column of results (Table 8, Column A), we replicate our main analysis, extending it to a broader setting, regressing contemporaneous *ROA* on *MWIC*. The coefficient on *MWIC* is negative and economically significant with a coefficient of −0.01 (t-statistic = −2.19), suggesting that *ROA* is an average of 1 percent lower for firms with ineffective internal controls after controlling for prior performance and other determinants of operating performance, such as growth.

In our second analysis (Table 8, Column B), we explore whether, among firms with ineffective ICFR in year *t*, those that remediate their ineffective ICFR in year $t+1$ experience an improvement in *ROA* (measured from year *t* to $t+2$, consistent with our prior analyses). We find a positive coefficient on *Remediation-MWIC* (coefficient = 0.026), indicating that following the remediation of ineffective internal controls, firms experience, on average, a significant improvement of 2.6 percent in *ROA* from year *t* to $t+2$ (t-statistic = 1.56; one-tailed p-value = 0.060).

Finally, in Table 8, Column C, we examine whether future ROA for firms that remediate MWIC in year *t* (n = 435) and firms that do not remediate MWIC in year *t* (n = 625 − 435 = 190) is statistically different from firms that report effective ICFR in year *t* (n = 8,852 − 625 = 8,227). We find that the 190 firms that disclose ineffective ICFR in year *t* have ROA that is 3.3 percent lower than the full sample (one-tailed $p\text{-value} < 0.05$).

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[20] As with our inventory-related MWIC, these weaknesses are flagged under Section 404, indicating that they violate the assumptions of effective ICFR, but can also threaten more general internal control effectiveness.

TABLE 8

The Association between Operating Performance and Material Weaknesses in Internal Control over Financial Reporting

	Dependent Variable =					
	Column A		Column B		Column C	
	Pred. Sign	ROA_t	Pred. Sign	$\Delta ROA_{t,t+2}$	Pred. Sign	ROA_{t+2}
Intercept		−0.105***		−0.006		−0.065***
		(−12.95)		(−0.17)		(−4.11)
MWIC	−	−0.010**				
		(−2.19)				
Remediation-MWIC			+	0.026*	?	0.009
				(1.56)		(1.55)
NoRemediation-MWIC					−	−0.033**
						(−1.68)
CapitalIntensity		−0.003***		−0.022		0.001
		(−3.26)		(−1.55)		(0.27)
SalesVolatility		−0.004		0.008		−0.037
		(−0.53)		(0.08)		(−1.52)
Growth		−0.013**		0.041**		−0.018
		(−2.44)		(2.56)		(−1.51)
Complexity		−0.001		−0.013		0.003
		(−0.93)		(−0.76)		(1.20)
ForeignOperations		0.000		0.012		0.002
		(0.18)		(0.49)		(0.55)
Age		0.003**				0.004*
		(2.56)				(1.77)
Auditor		−0.007		0.049		0.017
		(−1.23)		(1.23)		(0.91)
PriorROA		0.706***		−0.154**		0.692***
		(27.07)		(−2.11)		(10.14)
Loss		−0.005		−0.019**		0.017*
		(−1.17)		(−1.97)		(1.85)
Size		0.012***		0.111***		0.007***
		(10.80)		(4.21)		(2.93)
Year Indicators		Included		Included		Included
Industry Indicators		Included		Included		Included
Firm-Year Observations		8,953		625		8,852
MWIC Observations		640		625		625
Remediation-MWIC				435		435
F-value		683.1		12.81		155.28
Adjusted R^2		0.549		0.221		0.229

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*, **, *** Denote p-values of less than 0.10, 0.05, and 0.01, respectively (one-tailed if there is a sign prediction, two-tailed otherwise).

Columns A and C include all firm-year observations with available data; Column B includes only firm-year observations of firms with MWIC in year *t*. *ROA* is return on assets, defined as earnings before extraordinary items scaled by total assets. $\Delta ROA_{t,t+2}$ is the difference between $ROA_{t+2} - ROA_t$, where *t* is the last year the firm reported an MWIC. Control variables in Column A are measured at $t-1$. Control variables in Column B except *PriorROA* are change variables, defined as $(t+2) - t$. Control variables in Column C are measured at $t+1$. Standard errors are clustered by firm, and t-statistics are in parentheses. Other variable definitions appear in Appendix A.

Overall, the results of the more general analysis assessing the relation between MWIC and firm operating performance as proxied by ROA are consistent with our main analysis examining the relation between inventory-related MWIC and indicators of inventory management known to affect operating performance. We conclude that firms that maintain ineffective ICFR have weaker operating performance, both concurrently and prospectively, and firms that remediate their ineffective ICFR experience improvements in their operating performance.

Sensitivity Tests

Accuracy of Inventory Transactions versus Reported Ending Inventory Value

Our inventory turnover ratios and inventory impairments are a product of the accounting system. The premise of our paper is that auditors can correct the inventory values reported in the annual financial statements via substantive testing (e.g., Hogan and Wilkins 2008; Doyle et al. 2007b), but that the daily, weekly, and monthly inventory transactions will be affected by inventory-related MWIC. It is possible, however, that inventory values reported in firms' financial statements are erroneous, and that it is the misstated ending value of inventory that is affecting the association between inventory management and inventory-related MWIC. To explore the sensitivity of our results to this issue, we exclude all firm-years that restated as of December 31, 2012, and reestimate Equation (1). Specifically, in untabulated results, we exclude 520 firm-years with misstatements, of which 41 relate to ineffective ICFR over inventory; results are quantitatively similar.

Performance: Determinant versus Consequence

A number of studies have provided evidence that poorly performing firms are more likely to maintain ineffective ICFR (i.e., performance is a determinant of material weaknesses in ICFR). In our study, we posit that poor operating performance is also a *consequence* of inventory-related MWIC. In our main analyses, we investigate our hypothesized effect by controlling for non-inventory-related MWIC and also by conducting a change analysis. In this section, we explore this further by reestimating Table 4 solely among loss firms, and also by ROA quartile. Untabulated results are similar in each of these subsets of observations, further corroborating that the lower inventory turnover ratios we document are not simply a function of poor prior performance. Instead, we find additional evidence that poor operating performance is a consequence of inventory-related MWIC.[21]

Ending versus Average Inventory

Our inventory turnover ratios are scaled by average inventory. However, the results of our impairment tests suggest that the beginning balance of inventory may contain some soon-to-be-impaired inventory for firms reporting inventory-related MWIC, which would result in systematically lower inventory turnover ratios. This possibility biases in favor of finding a negative coefficient on *Inventory-MWIC* in the inventory turnover ratio test. As another untabulated robustness check, we repeat our analyses using firms' ending inventory balance. Results are qualitatively and quantitatively similar.

Alternate Samples

Although we used a matched sample for our impairment analysis because inventory impairments must be hand-collected from firms' financial statements, our main control sample for the inventory turnover analysis includes all firms on Compustat having the necessary data to conduct our empirical tests. When we replicate our inventory turnover test (Equation (1)) using the matched sample of 139 inventory-related MWIC firms and 139 effective ICFR firms used in our impairment analysis, the untabulated coefficient on inventory-related MWIC remains negative and significant, albeit weaker than that presented in Table 3.

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[21] Our change analysis also helps to address this concern, suggesting that it is ICFR affecting performance, not *vice versa.*

We also conduct a sensitivity test using an alternative sample for our inventory impairment analysis. Specifically, we estimate Equations (2) and (3) using the impairment data of Allen, Larson, and Sloan (2013), who collect inventory impairments for the 2004 population of firms covered by Compustat. The untabulated results are qualitatively and quantitatively similar.

V. CONCLUSION

We examine the association between inventory-related material weaknesses in internal control (MWIC) and accounting-based measures of inventory management to draw inferences on whether internal control over financial reporting (ICFR) has spillover effects on firm operations. We hypothesize that firms with inventory-related MWIC are more likely to experience the real effects of stock shortages, excess inventory, and greater inventory obsolescence as a consequence of maintaining ineffective inventory-related ICFR, leading to lower inventory turnover and more inventory impairments. Consistent with these expectations, we document that firms with inventory-related MWIC, as identified in firms' required internal control reports, have systematically slower inventory turnover and have a higher likelihood and magnitude of inventory impairments. We also find evidence that inventory turnover ratios, sales, gross margin, and cash flows from operations improve when the weaknesses are remediated. Because firms with higher inventory turnover ratios and fewer inventory impairments are viewed to be operating more effectively, our findings suggest that an unintended benefit of maintaining effective ICFR is better operating performance.

The debate over the required public disclosure of MWIC continues. COSO issued a revised internal control framework in December 2011 emphasizing the importance of communicating internal control effectiveness to external stakeholders. Nevertheless, in October 2011 the U.S. Jobs Council recommended amending SOX to allow public companies with market valuations below $1 billion to opt out of Section 404 compliance (Miller 2011). Moreover, the average manager perceives no operational benefit from maintaining effective ICFR (Alexander et al. 2013). Proposal legislation may also be introduced in the House of Representatives to adjust the definition of accelerated filers such that firms with market capitalization of less than $250 million or annual revenues of less than $100 million will no longer be required to disclose MWIC (*Strategic Finance* 2012). These policy issues would benefit from evidence on the effect of ICFR on firm operations because managers responsible for both the decision to establish and maintain effective ICFR, as well as the daily operation of their firm, may underestimate the actual benefits from ICFR. We leave the issue of whether the costs of establishing and maintaining effective ICFR outweigh the combined reporting and operational benefits to future research.

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APPENDIX A

Variable Definitions

Variable Name	Definition
InventoryTurnover	Inventory turnover ratio measured as cost of sales (Compustat COGS) in year t divided by inventory (Compustat INVT) averaged over years $t-1$ and t (on a FIFO basis).
IndAdj-InventoryTurnover	The firm-specific inventory turnover ratio less the median inventory turnover ratio for the firm's industry, where industry is defined by two-digit SIC.
Inventory-MWIC	An indicator variable equal to 1 if the firm reports there is an inventory-related material weakness in internal control in year t, and 0 otherwise; (Audit Analytics NOTEFF_ACC_REAS_KEYS = 32, or NOTEFF_FINFRAUD_KEYS = 32, or NOTEFF_OTHER_REAS_KEYS = 32).
Revenue-MWIC	An indicator variable equal to 1 if the firm reports there is a material weakness in internal control over revenue recognition in year t, and 0 otherwise; (Audit Analytics NOTEFF_ACC_REAS_KEYS = 39, or NOTEFF_FINFRAUD_KEYS = 39, or NOTEFF_OTHER_REAS_KEYS = 39).
Other-MWIC	The number of material weaknesses in internal control in year t excluding those over inventory or revenue recognition (Audit Analytics NOTEFF_ACC_REAS_KEYS ≠ 32 or 39, and NOTEFF_FINFRAUD_KEYS ≠ 32 or 39, and NOTEFF_OTHER_REAS_KEYS ≠ 32 or 39).
GrossMargin	The gross profit ratio measured as (sales (Compustat REVT) − cost of sales (Compustat COGS)) divided by sales (Compustat REVT) in year t.
CapitalIntensity	The natural logarithm of gross property, plant, and equipment (Compustat PPEGT) at the end of year t.
SalesVolatility	The standard deviation of annual sales (Compustat REVT) divided by average total assets (Compustat AT) in years t and $t-1$ over the prior seven years (requiring at least three non-missing observations).
SalesGrowth	Sales growth from year $t-2$ to year $t-1$.
Segments	The natural logarithm of the total number of geographic and operating segments in year $t-1$.
ForeignSales	An indicator variable equal to 1 if the firm reports foreign sales (Compustat FCA) in year t, and 0 otherwise.
FirmAge	The natural logarithm of the number of years that a company is covered by CRSP at the end of year $t-1$.
Auditor	An indicator variable equal to 1 if the firm hires one of the six largest audit firms (Audit Analytics AUDITOR_FKEY ≤ 7), and 0 otherwise.
ROA	Earnings before extraordinary items (Compustat IB) in year $t-1$ divided by average total assets over year $t-1$ and year $t-2$.
Loss	An indicator variable equal to 1 if net income (Compustat NI) in year $t-1$ is less than zero, and 0 otherwise.
Size	The natural logarithm of market value of equity (Compustat PRCC_F × CSHO) at the end of year $t-1$.

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How Audit Reviewers Respond to an Audit Preparer's Affective Bias: The Ironic Rebound Effect

Michele L. Frank
Vicky B. Hoffman
University of Pittsburgh

ABSTRACT: Prior research suggests that audit seniors' judgments are sometimes biased by their affect toward (i.e., feeling of personally liking or disliking) client personnel. We examine how experienced audit reviewers respond when reviewing an audit preparer's judgment that appears to be biased by the preparer's affect toward a client's controller. In our experiment, reviewers are provided with a preparer's judgment that appears inconsistent with the audit workpapers. We then examine the effect of providing versus not providing reviewers with a cue about the preparer's positive or negative affect toward the controller. We find that despite reviewers' belief that affect biases a preparer's judgment, reviewers who are informed of the preparer's affect do not rely less on the preparer's judgment. Instead, they actually rely more on the preparer's judgment than do those who are not informed about the preparer's affect. This result is consistent with Wegner's (1994) ironic rebound effect, which predicts that sometimes when individuals are trying not to rely on information, they ironically rely on it more. Our findings suggest a potential limitation of the audit review process.

Keywords: *audit review process; bias; ironic rebound effect; affect.*

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I. INTRODUCTION

A significant amount of research suggests that audit seniors' (i.e., preparers') judgments may be biased by factors such as their affect toward (feelings of personally liking or disliking) client personnel.[1] However, research has not examined whether the audit review process corrects for this type of bias in preparer judgments. To address this issue, our study examines how experienced audit managers (i.e., reviewers) respond when reviewing an audit preparer's judgment that they suspect could be biased by affect. Specifically, we investigate whether reviewers are able to rely less on a potentially biased preparer judgment.

We focus on reviewers' response to being informed about a preparer's affect because research shows (Bhattacharjee and Moreno 2002; Kadous, Leiby, and Peecher 2013) that affect influences senior auditors' judgments even when it should not because the affect is not related to client competence or audit risk. If reviewers rely too heavily on preparers' judgments that are overly influenced by affect, then audit quality could be reduced. For example, assume that a preparer's feelings of personally liking client personnel (i.e., positive affect) result in the preparer reaching a judgment that is overly favorable to the client. If reviewers are inappropriately influenced by the preparer's judgment, then they may overlook a serious problem in the client's financial statements. Similarly, if a preparer's feeling of disliking client personnel (i.e., negative affect) causes the preparer to reach a judgment that is overly critical, and if reviewers are inappropriately influenced by the preparer's biased judgment, then reviewers may require unnecessary costly audit procedures. Thus, over-relying on a preparer's judgment that has been biased by affect could undermine the efficiency and effectiveness of the audit review process, resulting in significant costs to the audit firm. Indeed, Bhattacharjee and Moreno (2013) suggest that audit firms would benefit from research that investigates whether the review process can minimize the impact of judgment errors caused by preparers' feelings.

We develop competing hypotheses about how reviewers respond to a preparer's judgment when it appears to be biased by affect. When reviewers suspect that affect biased the preparer's judgment, making it too positive or too negative, but do not know the extent of the bias, reviewers will try to rely less on the preparer's judgment. Our competing hypotheses address whether reviewers actually do rely less on the preparer's judgment. The first hypothesis predicts that they do not. This hypothesis is based on psychology research that predicts that in settings such as ours, individuals are likely to exhibit the "ironic rebound effect," where they rely more on the biased judgment than they would if they had no reason to suspect a bias (Wegner 1994; Cain, Loewenstein, and Moore 2005; Silverman et al. 2010). In contrast, our competing hypothesis is based on audit review process literature and predicts that reviewers' judgments will not exhibit the ironic rebound effect. We propose competing hypotheses because despite researchers' speculation that professionals' judgments will be susceptible to the ironic rebound effect in settings such as ours, they have not empirically tested this idea (Cain et al. 2005; Silverman et al. 2010). Similarly, although the auditing literature (Libby and Trotman 1993; Ramsay 1994; Tan and Yip-Ow 2001) generally finds the review process to be effective at mitigating errors and biases, it has not examined a setting in which reviewers suspect a preparer's judgment is biased, but do not know the extent of that bias. Our study provides a test of these alternatives.

To test our hypotheses, we conduct an experiment in which audit managers and senior managers act as reviewers. All participants are provided with a preparer's judgment that appears inconsistent with the workpaper evidence. While the workpaper evidence includes a balanced set of positive and negative information, we manipulate the preparer's judgment to be either overly favorable or unfavorable to the client. We also manipulate whether reviewers are or are not provided with information about the preparer's affect toward the controller, and when such information is provided, we vary whether the preparer personally likes or dislikes the controller. We then examine how being informed of the preparer's affect influences reviewers' judgments.

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[1] Consistent with the psychology literature, we use the word "bias" to mean "prejudice in favor of or against one thing, person, or group compared with another, usually in a way considered to be unfair" (*Oxford Dictionaries* 2013).

Similar to prior auditing literature (Libby and Trotman 1993; Ramsay 1994), we find that reviewers who are *not* informed about the preparer's affect are not significantly influenced by the preparer's judgment and arrive at judgments more consistent with the audit evidence. In contrast, reviewers who not only receive the same preparer judgment and workpaper evidence, but who are also informed of the preparer's affect, are significantly influenced by the preparer's judgment. This finding that reviewers rely more on the preparer's judgment when they suspect that it is biased is consistent with the ironic rebound effect. We conduct a follow-up study that provides evidence that reviewers indeed believe that a preparer's positive (negative) affect toward the controller will cause the preparer's judgment to be more favorable (unfavorable) to the client than is warranted. Thus, despite their belief that affect biases the preparer's judgment, reviewers fail to mitigate this bias when reaching their own judgments.

These findings extend the audit review process literature by highlighting that reviewers might not always mitigate biases in preparers' judgments, particularly in cases where they cannot readily determine how much the bias influenced the preparer's judgment. The previous research that finds that reviewers rely less on a preparer's judgment that is inconsistent with workpaper evidence (Solomon 1987; Tan and Yip-Ow 2001) typically provides reviewers with enough information to determine how much a preparer's judgment deviated from an unbiased judgment. However, in the current study, reviewers cannot readily determine the extent to which the preparer's judgment is biased, which is often the case in practice. Our results suggest that in this type of setting, reviewers may be inappropriately influenced by the preparer's judgment.

Audit firms should be interested in these findings because they view the review process as a key quality control mechanism (Rich, Solomon, and Trotman 1997). If reviewers rely too heavily on a preparer's biased judgment, then they inadvertently increase the firm's audit risk. Identifying this potential limitation of the review process is a necessary first step in helping reviewers respond more appropriately when they believe it is likely that a preparer's judgment is biased.

Finally, our study is the first to demonstrate the potential importance of the ironic rebound effect in audit settings. The ironic rebound effect has been documented in other settings, such as with jurors who ultimately rely more on evidence they are told to ignore (Cox and Tanford 1989). However, we are not aware of any previous study that tests for the ironic rebound effect in professionals' judgments. Thus, we contribute to the psychology literature by providing evidence consistent with previous speculations that the ironic rebound effect will occur in a setting such as ours (Silverman et al. 2010; Cain et al. 2005).

In Section II, we develop our competing hypotheses. We explain our methodology in Section III, and present the results of our study in Section IV. Section V describes our supplemental analyses, including a description of our follow-up study, and Section VI concludes with a discussion of the implications of our research.

II. HYPOTHESIS DEVELOPMENT

Interpersonal Affect

When auditors interact with client personnel, they sometimes develop strong feelings of personally liking or disliking these individuals. These types of feelings, often referred to as interpersonal affect, have been described by Zajonc (1980) as being involuntary, effortless, and irrevocable. An auditor's personal feelings toward a client's controller could be relevant to the audit if the feelings are related to the client's competence or to factors such as trustworthiness that influence audit risk. However, to the extent that a preparer's feelings arise from factors that do not influence client competence or audit risk, these feelings should not influence auditors' judgments.[2]

Considerable evidence (Regan, Straus, and Fazio 1974; Katz and Glass 1979; Tsui and Barry 1986; Kadous 2001; Kida, Moreno, and Smith 2001; Bhattacharjee and Moreno 2002; Moreno, Kida, and Smith 2002) suggests that even when it is irrelevant, interpersonal affect can influence individuals' decisions. For example, Bhattacharjee and Moreno (2002) find that irrelevant negative affect unrelated to client competence or to perceived audit risk influences less experienced auditors' (staff and seniors) inventory obsolescence judgments. Our study extends these prior studies by examining how reviewers respond to being informed about a preparer's affect.[3] We also examine how reviewers respond to both a preparer's positive and negative affect. While Bhattacharjee and Moreno (2002) examine only feelings of disliking the client, subsequent research finds that feelings of liking client personnel can also influence preparers' judgments even when they should not (Kadous et al. 2013; Bhattacharjee, Moreno, and Riley 2012).

We expect reviewers in our study who are informed of a preparer's affect to suspect that it biased the preparer's judgment. Psychology research shows that individuals recognize that people's judgments are biased by their feelings. For example, individuals understand that others tend to be biased in favor of their in-group (Vivian and Berkowitz 1992) and against their out-group (Robinson, Keltner, Ward, and Ross 1995; Ehrlinger, Gilovich, and Ross 2005), and are aware that people's feelings of like or dislike could bias their judgments (Mills and Grant 2009). Our primary study assumes that reviewers believe a preparer's affect will bias the preparer's judgments, and we confirm this assumption in the follow-up study described in Section V.

Competing Hypotheses

Our main research question examines whether reviewers exhibit an ironic rebound effect when reviewing a preparer's judgment that they believe could be biased by affect. The review process literature finds mixed results regarding reviewers' response to biased preparers' judgments (Ricchiute 1999; Yip-Ow and Tan 2000; Tan and Yip-Ow 2001; Tan and Trotman 2003).[4] These mixed results likely occur because of differences in the nature of the information provided to participants. Specifically, studies such as Tan and Yip-Ow (2001) provide reviewers with enough information to determine both that the preparer is biased and the extent to which the bias influenced the preparer's work. In contrast, other studies, such as Ricchiute (1999), do not give reviewers any information that would allow them to infer that the preparer could be biased, so reviewers have no reason to believe they should rely less on the preparer's judgment. Our study more closely models what occurs in audit practice by providing contextual information about the preparer's affect toward the controller without explicitly informing reviewers that the preparer's judgment is biased or how much it might be biased.[5]

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[2] Factors unrelated to client competence or audit risk often cause an affective response. For example, positive affect may arise because of another individual's warmth, sociability, or happiness, even if that individual is not particularly competent (Asch 1946; McAllister 1995; Casciaro and Sousa Lobo 2005).

[3] In a subsequent study, Bhattacharjee et al. (2012) vary the level of client competence at the extremes of very high versus very low, and find that affect has a larger influence on preparers' judgments when the client has very low competence. Bhattacharjee et al. (2012) make the extreme levels of client competence very salient to the preparer participants and do not examine the moderate case used in the Bhattacharjee and Moreno (2002) study. We hold client competence constant at a moderate level (as in Bhattacharjee and Moreno 2002) and find that our reviewers believe that preparers will be influenced by affective information, consistent with the results of Bhattacharjee and Moreno (2002).

[4] Peecher, Piercey, Rich, and Tubbs (2010) also examine audit reviewers' judgments. However, they examine whether audit reviewers, who are themselves biased and cause the audit preparer to become similarly biased, anticipate and correct for their influence on an audit preparer's judgment. Because Peecher et al.'s (2010) audit reviewers are the cause of the audit preparer's bias, it is not clear whether audit reviewers are able to anticipate and adjust for an erroneous influence that is unrelated to them.

Cain et al. (2005) and Wegner (1994) offer insights into how reviewers might respond when reviewing a preparer's judgment that appears biased by affect. Cain et al. (2005) explain that individuals are likely to recognize when information appears biased and to try to reduce the relative weight they place on that information in arriving at their own judgment. Wegner (1994) suggests that when individuals do not have a clear understanding of how much to discount information, they ironically rely on it *more* than if they were not actively trying to rely on it *less*.

According to Wegner's (1994) model, attempting to ignore or rely less on information engages a subconscious monitoring process. This monitoring process helps ensure that individuals do not rely on information that they do not want to influence their judgments. However, when individuals are relatively uncertain about how much they should discount that information, their monitoring process becomes more activated as they work harder to ensure they are not relying on it too heavily. This increase in monitoring activity keeps the information one is trying not to rely on salient in working memory. Because more salient information has a stronger influence on judgments (Bargh 1989), an ironic rebound effect is likely to occur.

The ironic rebound effect has been found in many other settings (Cox and Tanford 1989; Wegner, Erber, and Zanakos 1993; Wegner 1994; Lieberman and Arndt 2000). For example, Cox and Tanford (1989) find that jurors who are told to disregard a piece of evidence ironically use that evidence more than do those who are not trying to avoid using the evidence. As explained in Lieberman and Arndt's (2000) discussion of Cox and Tanford's (1989) study, it is likely that the jurors were unsure how to respond to the abundance of information they were provided, which probably increased the intensity of their monitoring processes. Consequently, the inadmissible evidence became more salient and had a stronger influence on the jury's subsequent judgment.

Silverman et al. (2010) speculate that even professionals often lack a clear understanding of how to respond to biased information. They examine how physicians respond when asked to make a prescription decision based on a drug company-sponsored research study. The authors find that a majority of participants perceive a conflict of interest and believe they should adjust for it, but do not. Silverman et al. (2010) suggest that the physicians do not prescribe the drug less often because they do not know how much to discount the biased information in reaching their judgments.[6] Cain et al. (2005) also speculate about how individuals could respond when reviewing biased advice. They argue that the ironic rebound effect would be especially likely to occur when individuals are trying not to rely on a piece of advice or a judgment that appears to have been influenced by a subconscious bias, such as when the advice is received from an advisor with a conflict of interest.[7] Both Cain et al. (2005) and Silverman et al. (2010) address how people might respond to information that appears subconsciously biased by an advisor's conflict of interest. Their settings are quite similar to ours because individuals are likely to understand the probable direction of the subconscious bias, but to have difficulty knowing exactly how to respond to correct for the bias's effect on another individual's judgment.

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[5] Tan and Yip-Ow (2001) provide reviewers with all possible workpaper information so they can readily determine when preparers strategically omit specific facts from their workpaper documentation. We inform the reviewers that the evidence documented in the preparer's workpaper represents all available audit evidence. Even though research suggests that reviewers do not spontaneously consider that preparers sometimes stylize their workpapers (Ricchiute 1999), we wanted to further ensure that reviewers in our setting would not perceive that the preparer strategically documented evidence.

[6] Because the physicians in that study were given the option of not making a judgment and could respond that they were unsure whether to prescribe the drug, Silverman et al.'s (2010) study was not designed to test for the ironic rebound effect. Thus, it is unclear whether the physicians would over-rely on the biased research report if they had to make a prescription decision.

[7] Of course, an advisor's conflict of interest can also result in a conscious bias, but Cain et al.'s (2005) discussion focuses on subconscious biases that research finds are unintentional and not easily corrected.

In our audit setting, if reviewers do not know exactly how to discount a preparer's judgment that appears biased, then their monitoring process is likely to stay actively engaged. Specifically, when aware of a preparer's positive (negative) affect, reviewers will monitor themselves to ensure that their judgments are not too positive (negative). This monitoring process keeps the preparer's judgment salient in the reviewers' working memory, subconsciously increasing the weight reviewers place on the preparer's judgment relative to those who are not informed about the preparer's affect. Thus, reviewers in our setting may be susceptible to the ironic rebound effect.

As noted earlier, no research directly tests whether professionals, such as auditors, actually exhibit ironic rebound effects and whether reviewers can mitigate a suspected bias in a preparer's judgment when the extent of that bias is unknown. Prior auditing literature finds that the review process is often effective because reviewers ensure that judgments are consistent with the audit evidence and correct for inconsistencies between preparers' judgments and evidence (Libby and Trotman 1993; Ramsay 1994; Tan and Yip-Ow 2001). While the review process research does not examine how reviewers respond when they perceive a preparer's judgment was influenced by a subconscious bias with an indeterminate effect, it does suggest that reviewers would not rely more on a judgment when they perceive it is biased.

Reviewers could respond in two ways if they are *not* susceptible to the ironic rebound effect. First, they could mitigate the preparer's bias by reaching the same judgments when aware of the preparer's affect as when they are not aware of the preparer's affect. Second, reviewers not susceptible to the ironic rebound effect could "become even more vigilant" and "place less weight on the preparer's conclusions" (Tan and Yip-Ow 2001, 668) when they are informed about a preparer's affect. Thus, reviewers informed about a preparer's affect, but not susceptible to the ironic rebound effect, could rely on a preparer's judgment to the same extent or less than reviewers who are not informed about a preparer's affect.

Hypotheses 1a and 1b offer competing predictions about whether the ironic rebound effect will occur, with H1a predicting that it will and H1b predicting that it will not:

> **H1a:** Audit reviewers who are aware of a preparer's positive (negative) affect will reach judgments that are more favorable (more unfavorable) to the client than those who do not receive affective information.
>
> **H1b:** Audit reviewers who are aware of a preparer's positive (negative) affect will reach judgments that are the same as, or more unfavorable (more favorable) to the client than those who do not receive affective information.

III. METHOD

Participants

Participants were 119 audit managers and senior managers with an average of 9.37 years of audit experience recruited from two Big 4 accounting firms.[8] We chose participants with this level of experience because prior research shows that audit managers and senior managers are not influenced by irrelevant affective information when acting as preparers (Bhattacharjee and Moreno 2002). Furthermore, they are the ones who review the work of less experienced auditors (audit staff and audit seniors), who have been shown to be erroneously influenced by affect.

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[8] Because participant characteristics and results do not differ significantly between firms, we combine the two firms in all subsequent analyses.

Experimental Design

The study was conducted online. We emailed individuals identified by their firm as having the requisite experience levels. After agreeing to participate, participants were told to assume the role of an audit manager assigned the task of reviewing certain inventory judgments made by a hypothetical audit senior. Each participant was then randomly assigned to one of four treatment conditions and sent a link to a website that contained the task materials.

We employ a 2 × 2 between-subjects design where we manipulate (1) the amount of the preparer's recommended inventory writedown (Positive or Negative), and (2) whether the reviewer is informed about the preparer's affect toward the controller (Affective Information Provided or Affective Information Not Provided). In the Positive conditions, the preparer reaches a very favorable conclusion, recommending that no writedown is required, whereas in the Negative conditions, the preparer reaches a very unfavorable conclusion, recommending an extremely high writedown of \$1.2 million. We construct cases such that preparers make judgments consistent with their affective response. That is, when the preparer personally likes the controller, he or she reaches a favorable judgment, and those who dislike the controller only reach an unfavorable judgment. We make this design choice because previous literature (Bhattacharjee and Moreno 2002; Kadous et al. 2013) indicates that individuals' judgments tend to be influenced in a direction consistent with their affective reactions.

In the Affective Information Provided conditions, reviewers are told about either the preparer's strong positive personal feelings (Positive Affect) or strong negative personal feelings (Negative Affect) toward the controller. For each affect condition, we create a corresponding control condition. In the Positive (Negative) Control condition, all information is the same as in the Positive (Negative) Affect condition, except that no affective information is provided. We include two separate control conditions to avoid varying two variables at once—whether affective information is provided and the dollar amount of the preparer's recommended writedown. Specifically, in both the Positive Affect and Positive Control conditions, the preparer recommends a \$0 writedown, whereas in the Negative Affect and Negative Control conditions, the preparer recommends a \$1.2 million writedown.[9] Comparing each affect condition to its control condition isolates the influence of the reviewer knowing about the preparer's affect.

Experimental Task

After being told they were the reviewers on the engagement, participants received a narrative providing information about the company's background and the firm's past and present audits.[10] The background information indicated that client personnel were competent and that the audit firm had issued unqualified opinions in each of the last five years. They were also informed that the client's internal controls, including inventory controls, were effective and that there have never been any disputes between the audit firm and its client with respect to financial disclosures or applications of accounting principles. Finally, participants were informed that they had already reviewed the preparer's inventory procedures, and that other than the valuation issue, the inventory balance was found to be free of material misstatements and omissions.

The background information included an affect manipulation. Participants in the Positive Affect condition were told that when asked about how the audit was going, the preparer mentioned that he was happy to work on this engagement because he had previously worked with the client's controller when that controller worked at a different company. Based on their prior interactions, he really liked the controller and found him to be very pleasant to work with. Conversely, in the Negative Affect condition, the preparer said he was not looking forward to working on the engagement because, based on their prior interactions at another company, he found the controller arrogant, condescending, and extremely unpleasant to work with. This manipulation was intended to convey that the preparer's feelings were strong enough that he was inclined to mention them when asked only about how the audit was going. Presumably, preparers often have some personal feelings toward client personnel, but would not be inclined to mention them unless they were relatively strong.

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[9] In the Positive conditions participants were told the preparer recommended an inventory writedown amount of \$0, leaving the \$1.8 million preliminary inventory balance unadjusted. In the Negative conditions, participants were told the preparer recommended an inventory writedown amount of \$1.2 million, adjusting the initial \$1.8 million balance down to an adjusted balance of \$600,000. The online task was programmed so that when a participant entered a recommended amount of writedown to inventory, the computer automatically calculated the revised inventory balance such that participants did not have to do this calculation themselves.

[10] We created the narrative by modifying a portion of the task described in Bhattacharjee and Moreno (2002).

By design, the preparer's relationship with the controller was based on their interactions at a different company to avoid unintentionally providing participants in the affect conditions with additional information about the company's current audit situation. Furthermore, as confirmed by our post-experimental questionnaire (PEQ), reported in the "Results" section, the additional paragraph about affect did not provide any information that increased the risk associated with the audit, nor did it influence the perceived competence of the client's controller.

Participants were next told that, in a prior meeting, they had asked the preparer for a workpaper that presented his recommended inventory writedown amount and a written summary of the evidence used to make his judgment. Participants in all conditions received the same written summary of evidence from the preparer (see Appendix A). Two partners from a Big 4 audit firm reviewed all case materials, and indicated that the information provided was realistic and typical of information an auditor would encounter during an audit of inventory. Both individuals also agreed that (1) the evidence in the workpaper included a mix of information, some of which was consistent with inventory obsolescence and some that was inconsistent, making the task fairly subjective, and (2) the appropriate inventory writedown was greater than $0, but less than $1.2 million.

After reviewing the workpaper, participants were asked if they agreed with the preparer's recommended inventory writedown amount. Participants who disagreed were then asked to provide what they believed was the appropriate inventory writedown amount. They were able to go back and review both the background information and the preparer's workpaper when making their judgments, but not when answering the subsequent PEQ. Finally, participants answered manipulation check questions and questions designed to gather demographic information.[11]

IV. RESULTS

Manipulation Checks and Audit Reviewers' Perceptions of Competence and Audit Risk

Table 1 presents means (standard deviations) for the manipulation check questions. Participants in the Positive Affect and Positive Control (Negative Affect and Negative Control) conditions were asked whether it seemed the preparer personally liked (disliked) the controller and were asked to select "Yes," "No," or "Do Not Know." Table 1, Panel A shows that almost all participants correctly answered about the preparer's affective response to the controller (mean = 92.44 percent).[12]

Also, participants attended to the information presented, and perceived client personnel to be relatively competent and audit risk (i.e., risk of misstatement) to be relatively low. Specifically, in

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[11] These questions primarily addressed months of audit experience, the participant's gender, the participant's job title (either senior manager or manager), and the amount of time the participants had spent auditing manufacturing clients.

[12] A Chi-square test confirms that participants' responses were strongly associated with the experimental condition to which they were assigned ($\chi^2(\text{df} = 3, n = 119) = 86.5, p < 0.001$). Three of the 35 participants in the Positive Affect condition and four of the 30 participants in the Negative Affect condition answered "Do Not Know" instead of providing the correct response of "Yes." Excluding those participants who answered this item incorrectly does not change any of our statistical inferences.

TABLE 1

Summary of Responses to Manipulation Checks by Condition

Panel A: Percentage of Participants Who Correctly Identified Preparer's Affect[a]

Condition	% Correct Response
Positive Affect	91.43%
Positive Control	96.00%
Negative Affect	86.67%
Negative Control	96.55%
Overall	92.44%

Panel B: Mean (Standard Deviation) of Participants' Responses Regarding Client Competence and Audit Risk

	Positive Affect	Positive Control	Negative Affect	Negative Control	Overall
Agree that Client is Competent[b]	8.29	7.52	8.10	8.28	8.08
	(1.69)	(1.91)	(1.57)	(1.73)	(1.73)
Agree that Audit Risk is Low[c]	8.05	7.36	8.00	7.34	7.72
	(2.37)	(2.58)	(2.13)	(2.62)	(2.41)

[a] Participants in the Positive Affect and Positive Control (Negative Affect and Negative Control) conditions were asked whether the audit senior personally liked (disliked) the client's controller, and responded "Yes," "No," or "Do Not Know."

[b] Participants were asked to assess the extent to which they agreed with the assertion that the client's accounting personnel, including the controller, were competent. Participants responded on an 11-point scale, from 0 "Completely Disagree" to 10 "Completely Agree," with a midpoint of 5 "Neutral."

[c] Participants were asked to assess the extent to which they agreed with the assertion that other than the valuation issue, the client's inventory balance was free of misstatements or omissions. Participants responded on an 11-point scale, from 0 "Completely Disagree" to 10 "Completely Agree," with a midpoint of 5 "Neutral."

Table 1, Panel B, on an 11-point scale where 0 = Completely Disagree, 5 = Neutral, and 10 = Completely Agree, participants agreed that the client's accounting personnel, including the controller, were competent (mean = 8.08) and that, other than there being a question of valuation, the client's inventory balance was free of misstatements or omissions (mean = 7.72). There were no significant differences in any of these measures across treatment conditions.[13]

In addition, PEQ items examined whether reviewers perceive that affect could influence both their own judgment and preparers' judgments. Specifically, we asked participants in the Positive Affect and Positive Control conditions to assess the likelihood that their own feelings of liking a client would influence their audit judgments, and the likelihood that a staff or senior's feelings would influence their judgments. Participants in the Negative Affect and Negative Control conditions were asked identical questions regarding feelings of disliking a client.[14] Participants responded on an 11-point scale, from 0

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[13] We conducted ANOVAs to confirm that there were no main effects or interactions for the manipulation check variables (all p values > 0.10). A series of nonparametric tests provided similar results.

[14] We combined the PEQ results of the Positive Affect and Positive Control conditions because participants were asked the same questions about liking the client, and because their responses did not differ from each other ($t = 0.15$, $p = 0.880$). Similarly, we combined the PEQ results of the Negative Affect and Negative Control conditions because participants were asked the same questions about disliking the client, and because their responses did not differ from each other ($t = 0.67$, $p = 0.509$).

"Very unlikely to affect judgment" to 10 "Very likely to affect judgment." Participants' responses suggest that reviewers believe their own judgments might be slightly influenced by their feelings of *liking* the controller (mean = 2.79), but that preparers' judgments (mean = 5.02) are significantly more influenced ($t = 6.86$, $p < 0.001$). Similarly, reviewers believe their own judgments are only slightly influenced by their feelings of *disliking* the controller (mean = 3.15), but that preparers' judgments (mean = 4.95) are significantly more influenced ($t = 6.68$, $p < 0.001$). These responses also indicate that there is no significant difference ($t = 0.18$, $p = 0.859$, two-tailed) between the perceived magnitude of the effects on preparer judgments of liking versus disliking client personnel.

While the PEQ responses suggest that reviewers perceive that affect influences preparers' judgments, they do not address whether reviewers believe affect erroneously causes preparers to be more favorable or unfavorable to the client than warranted. We designed our follow-up study described in Section V to provide evidence on this issue. The results demonstrate that in a setting identical to that of our primary experiment, reviewers perceive that preparers' positive (negative) affect toward the controller erroneously makes their judgments more favorable (unfavorable) to the client than warranted.

Tests of H1a versus H1b

Our hypotheses offer competing predictions about whether reviewers' judgments exhibit the ironic rebound effect. The primary dependent measure is the inventory writedown amount agreed to by reviewers. Table 2, Panel A presents descriptive statistics, and Figure 1 illustrates these results graphically. To test whether the data are consistent with H1a or H1b, we report an ANOVA in Table 2, Panel B. The dependent variable is the amount of the reviewer's writedown, and the independent variables are whether any affective information was provided (Affective Information Provided versus Affective Information Not Provided) and the preparer's recommended writedown (Positive/$0 writedown or Negative/$1.2 million writedown).

The ANOVA results in Table 2, Panel B indicate that Affective Information and the Preparer's Recommendation have a significant interactive effect on Reviewers' Recommended Writedown ($F = 7.41$, $p < 0.01$). Figure 1 shows that this interaction is consistent with H1a, but not H1b. That is, reviewers' judgments are more consistent with preparers' judgments in the affect conditions than in their corresponding control conditions. It is important to note that this interaction arises because the affect-consistent response moves reviewers' judgments in two different *directions* (i.e., more positive versus more negative), not because the two types of feelings influence reviewers' judgments to a different extent.[15]

Because there is a significant interaction, it is not meaningful to look at the ANOVA's marginal main effects. Therefore, we conduct tests of simple main effects and present the results in Table 2, Panel C. H1a predicts that when reviewers know that a preparer has strong personal feelings, reviewers who are aware of a preparer's positive (negative) feelings will recommend a more favorable (unfavorable) writedown than will those not provided with affective information. Consistent with this prediction, the data reported in Table 2, Panel A and in the first simple main effects test in Panel C show that the mean reviewer writedown of $42,400 in the Positive Affect condition is more favorable (i.e., a smaller writedown) than the mean reviewer writedown of $119,200 in the Positive Control condition ($F = 5.49$, $p < 0.\ 03$, two-tailed). Similarly, the second simple main effects test indicates that the mean reviewer writedown of $357,333 in the Negative Affect condition is more unfavorable (i.e., a larger writedown) than the mean reviewer writedown of $183,103 in the Negative Control condition ($F = 4.07$, $p < 0.05$, two-tailed). These results are consistent with the ironic rebound effect predicted in H1a.

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[15] We later report in Table 3 the results of testing whether the effects of positive and negative affect are symmetric by comparing the absolute value of the change for positive versus negative affect.

TABLE 2

Tests of Hypotheses

Panel A: Mean (Standard Deviation) of Reviewers' Recommended Writedown

	Information About Preparer's Affect:	
Preparer's Recommended Writedown Amount	**Not Provided**	**Provided**
Positive Conditions (Writedown of $0 is Favorable to Client)	Positive Control $119,200 ($157,107) n = 25	Positive Affect $42,400 ($96,359) n = 35
Negative Conditions (Writedown of $1.2 million is Unfavorable to Client)	Negative Control $183,103 ($296,155) n = 29	Negative Affect $357,333 ($362,358) n = 30

Panel B: Two-Way ANOVA

Source	F	p-value (two-tailed)
Affective Information Provided (Yes or No)	0.55	0.458
Preparer Recommendation (Favorable/$0 writedown or Unfavorable/$1.2 million writedown)	19.16	<0.001
Affective Information × Preparer Recommendation	7.41	0.008

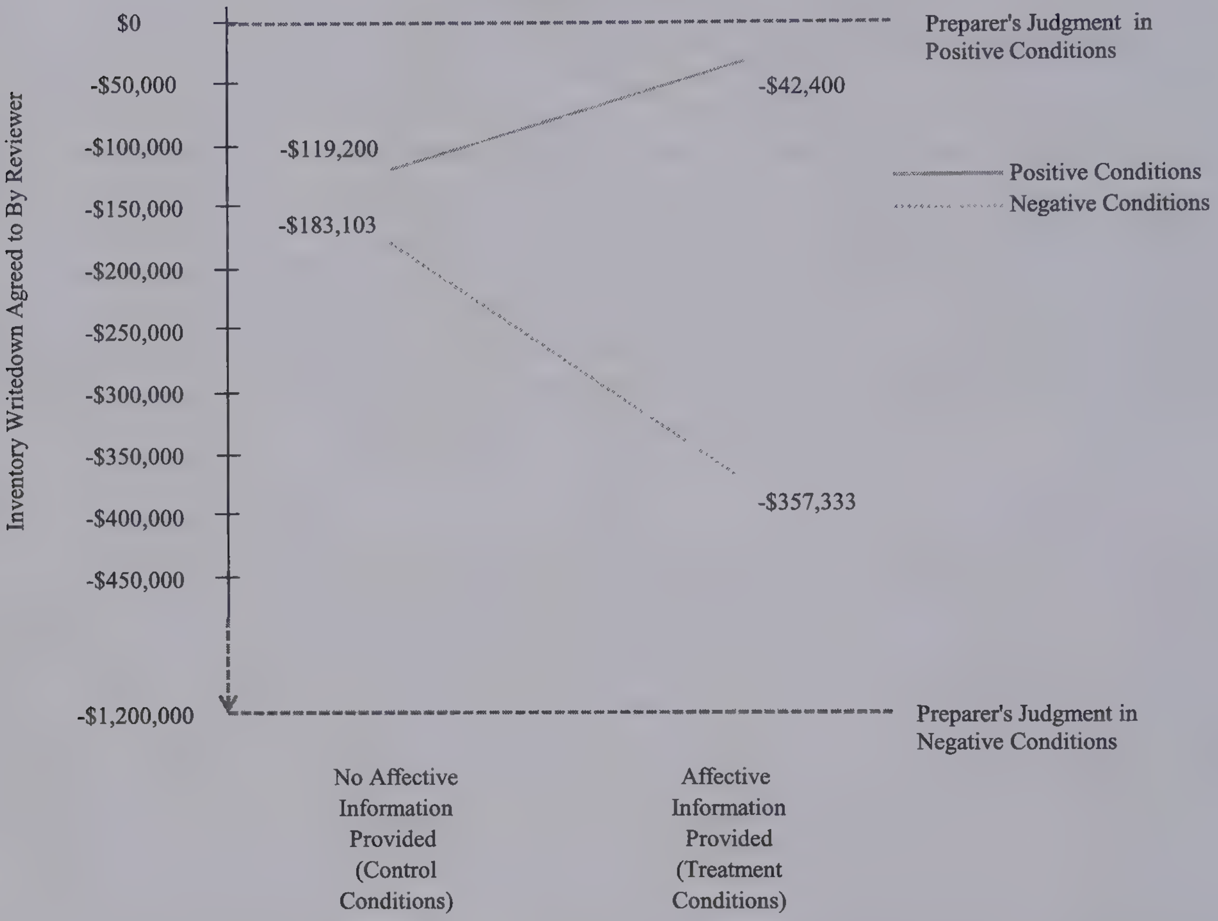
Panel C: Simple Main Effects

Source	F	p-value (two-tailed)
1. Effect of Providing Positive Affective Information When Preparer Recommends Favorable/$0 Writedown (Positive Affect versus Positive Control)	5.49	0.023
2. Effect of Providing Negative Affective Information When Preparer Recommends Unfavorable/$1.2 Million Writedown (Negative Affect versus Negative Control)	4.07	0.048
3. Effect of Preparer Recommendation (Positive Control versus Negative Control)	0.94	0.338
4. Effect of Nature of Affective Information (Positive Affect versus Negative Affect)	24.48	<0.001

In the Positive (Negative) Affect condition, reviewers were told that the preparer personally liked (disliked) the client.

Our hypotheses predict *differences* between the Affect conditions and the Control conditions, not the *level* of the reviewers' judgments in each condition. However, prior audit review process literature, such as Libby and Trotman (1993), suggests that reviewers in the Control conditions will not rely on a preparer's judgment that appears inconsistent with the audit evidence. The fact that reviewers' judgments are not different ($F = 0.94$, $p > 0.33$) between the Positive Control ($119,200) and Negative

FIGURE 1
Reviewers' Recommended Writedown to Inventory[a]



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[a] The mean reviewer writedown is reported in Table 2, Panel A. Figure 1 presents this reduction to the inventory balance as a negative number to better illustrate the results graphically. In the Negative Conditions, the preparer recommends a writedown of $1.2 million and in the Positive Conditions, the preparer recommends a writedown of $0.

Control ($183,103) conditions, as shown in the third simple main effects test in Table 2, Panel C, indicates that reviewers do not rely on preparers' judgments when they do not suspect the judgments are biased. Reviewers' inappropriate reliance on the preparer's judgment only occurs when the preparer's judgment appears biased by his or her affect. The judgments in the Control conditions also suggest that reviewers believe the appropriate mean writedown for this situation falls somewhere between $119,200 and $183,103.

V. SUPPLEMENTAL ANALYSES

Differences in the Influence of Positive versus Negative Affective Information

We did not hypothesize any difference in the extent to which positive versus negative affective information would influence reviewer judgments. However, we conduct a supplemental analysis to determine whether the *magnitude* of the difference between the affect and control condition is different for positive versus negative feelings. To conduct this analysis, we calculate the absolute value of the difference between the preparer's and reviewers' recommendations, and use this as the dependent variable in the ANOVA presented in Table 3. We find a main effect of providing reviewers with affective information ($F = 16.99$, $p < 0.001$), but that there is no interaction between

TABLE 3

Supplemental Analysis
Comparison of Magnitudes of Influence of Positive versus Negative Affect on Reviewers' Writedowns

Panel A: Mean (Standard Deviation) of Absolute Value of Difference between Reviewers' Writedown and Preparer's Recommendation

	Information About Preparer's Affect:	
Preparer's Recommended Writedown Amount	**Not Provided**	**Provided**
Positive Conditions (Writedown of $0 is Favorable to Client)	Positive Control $119,200 = |$119,200 – $0| ($157,107) n = 25	Positive Affect $42,000 = |$42,400 – $0| ($96,359) n = 35
Negative Conditions (Writedown of $1.2 million is Unfavorable to Client)	Negative Control $1,016,897 = |$183,103 – $1,200,000| ($296,155) n = 29	Negative Affect $842,667 = |$357,333 – $1,200,000| ($362,358) n = 30

Panel B: Two-Way ANOVA using Absolute Values

Source	**F**	**p-value (two-tailed)**
Affective Information Provided (Yes or No)	16.99	<0.001
Preparer Recommendation (Favorable/$0 writedown or Unfavorable/$1.2 million writedown)	338.27	<0.001
Affective Information × Preparer Recommendation	1.12	0.293

In the Positive (Negative) Affect condition, reviewers were told that the preparer personally liked (disliked) the client.

positive and negative affective information and the preparer's recommended writedown amount (i.e., $0 versus $1.2 million). This indicates that the magnitude of the influence of receiving positive affective information is not significantly different from that of receiving negative affective information ($F = 1.12$, $p = 0.293$). That is, reviewers seem to be influenced similarly by learning of a preparer's positive and negative affect.[16]

Audit Reviewers Expect Audit Preparers' Judgments to be Overly Influenced by Affect

As noted previously, we expected reviewers to recognize that preparers' affect influences their judgments, and the PEQ results confirm that they do. Note that such an influence might be warranted if reviewers attributed the affect to factors diagnostic of audit risk or client competence. However, affective reactions often capture traits like warmth and happiness, which should not influence preparers' judgments if they are unrelated to the audit.

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[16] The main effect of Preparer's Recommendation in Table 3 is due to the parameters chosen in the experiment. Because all participants believed the appropriate writedown was much closer to $0 than to $1.2 million, a much larger adjustment was needed when the preparer recommended an unfavorable writedown of $1.2 million than when the preparer recommended a favorable writedown of $0. This effect is unrelated to our hypotheses or results.

To assess whether reviewers perceive that a preparer's affect could influence the preparer's judgment to a greater extent than warranted, we conducted a follow-up study with participants similar to those in our primary experiment. We randomly assigned 40 Big 4 audit managers and senior managers with an average of 8.51 years of audit experience (none of whom participated in our primary experiment) to either a Positive or Negative condition, and sent the participants a link to access the case materials.

The materials asked participants to assume that they are the audit manager reviewing an engagement. The client background information and affect manipulation were identical to that of our primary experiment. That is, the Positive (Negative) condition's affect manipulation is the same as the experiment's Positive (Negative) Affect condition. After reading the background information, the materials informed participants that in a prior meeting, they had asked the preparer to make an initial judgment regarding the appropriate inventory writedown. The participants then rated the likelihood that the preparer's personal feelings toward the client's controller would erroneously influence the preparer's inventory writedown judgment. In the Positive condition, participants were asked to rate the likelihood that the preparer would erroneously reach a judgment that is *more favorable* to the client than warranted. Participants respond on an 11-point scale ranging from 0 percent (definitely not more favorable than warranted) to 100 percent (definitely more favorable than warranted). Similarly, in the Negative condition, participants rated the likelihood that the preparer would erroneously reach a judgment that is *less favorable* to the client than warranted, and responded on an 11-point scale ranging from 0 percent (definitely not less favorable than warranted) to 100 percent (definitely less favorable than warranted).

Table 4 presents a summary of participants' responses. As shown in Panel A, in the Positive condition, 100 percent of participants believe there is at least some likelihood that the preparer's feelings would erroneously influence his or her judgment to be more favorable to the client than warranted. In addition, 71.4 percent of the participants (15 of 21) believe it is *at least* 50 percent likely, and 42.8 percent (9 of 21) believe it is *more than* 50 percent likely that the preparer's judgment would erroneously be more favorable to the client than warranted. Panel B of this table shows similar results for the Negative condition, where 90 percent of participants (all but two) believe there is at least some likelihood that the preparer's feelings would erroneously influence his or her judgment to be more unfavorable to the client than warranted. In addition, 63.2 percent (12 of 19 participants) believe it is *at least* 50 percent likely, and 47.4 percent (9 of 19 participants) believe it is *more than* 50 percent likely, that the preparer's judgment would erroneously be more unfavorable to the client than warranted. Consistent with the primary experiment, there is no difference between mean responses in the Positive and Negative conditions ($t = 1.01$, $p = 0.32$). Thus, our follow-up study confirms that most experienced auditor reviewers (38 of 40, or 95 percent) perceive that preparers' affect will have some erroneous influence on preparers' judgments.

VI. CONCLUSIONS AND IMPLICATIONS

We report the results of an experiment designed to investigate how experienced auditors acting as reviewers respond when reviewing a preparer's judgment that they do or do not perceive might be biased by affect. We find that reviewers in the control conditions, who are not provided with information about the preparer's affect, are able to avoid relying too heavily on the preparer's judgment, and instead make judgments based primarily on the workpaper evidence. This is consistent with the results of prior audit review process studies that find that reviewers mitigate

TABLE 4

Results of Supplemental Study to Assess Reviewers' Perceptions of the Influence of Affect on Preparer Judgments

Panel A: Positive Condition (n = 21)

Response	Number of Participants	% of Total Participants
0% Likely	0	0.0%
10%–40% Likely	6	28.6%
50% Likely	6	28.6%
> 50% Likely	9	42.8%
	21	100.0%

In the Positive Condition, participants were told that the audit preparer personally likes the client's controller. Participants were then asked to rate the likelihood that the preparer's strong personal feelings toward the client would lead the preparer to erroneously reach a judgment that is *more favorable* to the client than warranted. Participants respond on an 11-point scale, ranging from 0 percent (definitely not more favorable than warranted) to 100 percent (definitely more favorable than warranted).

Panel B: Negative Condition (n = 19)

Response	Number of Participants	% of Total Participants
0% Likely	2	10.5%
10%–40% Likely	5	26.3%
50% Likely	3	15.8%
> 50% Likely	9	47.4%
	19	100.0%

In the Negative Condition, participants were told that the audit preparer personally dislikes the client's controller. Participants were then asked to rate the likelihood that the preparer's strong personal feelings toward the client would lead the preparer to erroneously reach a judgment that is *less favorable* to the client than warranted. Participants respond on an 11-point scale, ranging from 0 percent (definitely not less favorable than warranted) to 100 percent (definitely less favorable than warranted).

preparer errors and biases (Libby and Trotman 1993; Ramsay 1994). However, when reviewers in our study also receive information about the preparer's affect, they rely on the preparer's judgment more than when they are not informed about a preparer's affect. This finding is consistent with the ironic rebound effect, which predicts that when reviewers do not know exactly how to discount a preparer's judgment that appears biased, they will ironically rely on it more.[17] Our results suggest that reviewers can better adjust for a preparer's judgment that is simply inconsistent with the audit evidence than one that appears biased by the preparer's affect.

These results have implications for audit practice and audit research. Prior studies, such as Agoglia, Hatfield, and Brazel (2009), Rich et al. (1997), Ramsay (1994), and Libby and Trotman

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[17] The ironic rebound effect seems consistent with other research that suggests that when the cognitive demands of a task increase, as is the case when monitoring activity increases, it is more likely that individuals will anchor on a salient value (Kahneman and Frederick 2002).

(1993), demonstrate a number of benefits generally associated with the audit review process. However, we document a circumstance in which the review process does not seem as effective—when reviewers are faced with a preparer judgment that appears biased, but they do not know the extent of the bias or exactly how to adjust for it, they do not mitigate the bias. This finding could be relevant in many audit contexts because, as discussed in R. Ashton and A. Ashton (1995) and Bonner (2008), several other psychological factors have been found to bias preparers' judgments. If reviewers cannot mitigate these biases, then the audit review process's ability to function as a quality control mechanism could be impaired.

We document a specific potential problem with the audit review process. Future research could replicate these results and extend them by investigating ways to alleviate this problem and make reviewers less susceptible to the ironic rebound effect. For example, it may be possible to restructure the reviewer's task. In cases where reviewers perceive that a preparer's judgment is biased, it might be helpful to prompt reviewers to explicitly incorporate their perceptions of bias into the judgment process rather than to try to ignore the preparer's judgment (Wegner 1994; Lieberman and Arndt 2000). Restructuring the task in this way could put less demand on the reviewer's monitoring process.

Our experimental setting made the preparer's affective reaction toward the controller salient to the reviewer. One might question how reviewers on actual audits might become aware of a preparer's affect. Based on our discussions with practicing auditors, audit managers and partners often visit audit staff on client engagements, taking them to lunch or interacting with them in other informal social settings. In these settings, in addition to discussing audit issues, it is not unusual for preparers with relatively strong personal feelings toward the client's personnel to discuss them with the rest of the audit team. Moreover, psychology research about secondhand affect (Gilovich 1987; Baron, David, Brunsman, and Inman 1997) suggests that these types of discussions could lead reviewers to experience secondhand affective reactions, which are subconsciously positive toward client personnel that the preparer likes and negative toward those that the preparer dislikes. Future research could investigate whether reviewers experience secondhand affect, and if they do, if their secondhand affect exacerbates the results found here.

A potential limitation of our study relates to the fact that in practice, a reviewer who perceives that a preparer's judgment may be biased could go back and gather additional evidence or ask another preparer who does not have strong affect toward the client's controller to re-perform the preparer's work. However, it is unlikely that such significant steps would normally be taken given the potential immediate cost to the audit firm and the time constraints inherent in the audit process (Lambert and Agoglia 2011; Asare and McDaniel 1996). Furthermore, the auditing literature suggests that once reviewers are aware of the initial preparer's judgment, it may not be possible for them to completely ignore this judgment when reviewing the work of another preparer (H.-T. Tan and S.-K. Tan 2008).

Another potential limitation of our study is that we rely on final audit judgments to test our hypotheses without direct evidence regarding the subconscious cognitive processes underlying those judgments. We also have no evidence regarding the exact weight that reviewers placed on the preparer's judgment. Instead, like prior studies in this area, we rely on final judgments to demonstrate the effect. Despite not having direct evidence regarding the underlying cognitive process, we find it reassuring that the pattern of judgments is consistent with prior ironic rebound effect studies. Also, the fact that the results were the same in both the Positive and Negative between-subjects treatment conditions provides additional assurance that the effects we observe are systematic and did not occur by chance. Because research suggests that individuals generally lack insight into their subconscious cognitive processes (Nisbett and Wilson 1977), participants are unlikely to know how much weight they placed on the preparer's judgment or how much they discounted it. Future research could consider employing process tracing or magnetic imaging techniques to investigate the subconscious cognitive mechanisms that underlie these judgments.

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APPENDIX A

The following workpaper evidence, some of which was adopted from Bhattacharjee and Moreno (2002), was provided to all participants.

- BDRM has 60,000 electronic components in the year-end stock, carried at full absorption cost of $30 each. This is equivalent to six months' sales at 10,000 units per month. Over the last three years, the average selling price was $50 per unit and delivery costs were $9 per unit. Production of these units is continuing.
- During the year ended December 31, 2009, it was discovered that BDRM's primary competitor, Roth, Inc., developed a prototype of a product that may be viewed as technically superior to BDRM's. While BDRM has begun to develop a product comparable to Roth's, the production of its existing design will continue so that BDRM is able to serve its existing customer needs until the commercial success and cost competitiveness of this product is established.
- According to Roth's press releases, their new electronic component is currently expected to sell for approximately $40, which is below the price at which BDRM has historically offered its product. While BDRM is unsure whether Roth's lower price is a temporary marketing strategy, Company management believes that Roth's new device will be priced competitively when available. Consequently, management believes that significant pricing changes may be necessary for BDRM to sell existing inventory and continue production.
- BDRM believes that it is premature to project any significant losses on its electronic component inventory for many reasons. First, although Roth has recently begun accepting pre-orders, BDRM believes, given likely retooling and production delays, that it will take at least 18–20 months for Roth to gear up to full production. Furthermore, BDRM anticipates that many customers will be unwilling or unable to wait that long for an item that is unproven and has never been used in actual production conditions. BDRM management is also somewhat skeptical of the adequacy of Roth's testing of its new technology and, as such, believes that Roth may be attempting to market its device prematurely.
- BDRM has an international marketing team that aggressively markets its older technology products in developing nations around the world. Past experiences indicate that there is a healthy third-world market for electronic components such as the devices BDRM is currently manufacturing. Preliminary analysis by BDRM's international team indicates that 5,000 units per month could be expected, conservatively, to sell in these foreign markets by using existing market channels and personnel. The selling price of these products would likely yield positive profit margins.

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The Effect of Nonprofit Governance on Donations: Evidence from the Revised Form 990

Erica Harris
Rutgers, The State University of New Jersey, Camden

Christine M. Petrovits
The College of William & Mary

Michelle H. Yetman
University of California, Davis

ABSTRACT: We examine whether donors reward nonprofit organizations that report better governance. From a sample of 10,846 organizations from 2008 to 2010, we first identify seven nonprofit governance dimensions using factor analysis. We then test whether the quality of governance influences donor decisions by including the seven governance factors in the standard donor's demand model. We find consistent evidence that donations and government grants are positively associated with six of the seven factors that capture good governance, including formal written policies (e.g., conflict of interest), independent audits and audit committees, review and approval of executive compensation, board oversight (e.g., board independence), management characteristics (e.g., no related parties), and accessible financial information. Our results have implications for nonprofit managers and regulators. Moreover, mandatory *disclosure* of governance policies for nonprofit organizations provides an interesting contrast to mandatory *adoption* of governance policies for publicly traded companies.

Keywords: *nonprofit; corporate governance; disclosure; IRS Form 990; donations.*

JEL Classifications: *G18; G38; H39; L30; L31; L38; M40; M41; M42; M48.*

Data Availability: *All data are publicly available.*

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I. INTRODUCTION

This study investigates whether donors reward nonprofit organizations that exhibit higher-quality corporate governance. Annual charitable giving in the United States has grown to approximately $316 billion (Giving USA 2013). With the increase in size of the nonprofit sector, there has been a corresponding increase in the number of concerns about the misuse of charitable assets. Broadly speaking, nonprofit corporate governance refers to the set of internal and external mechanisms designed to ensure that managers are working to fulfill their organization's charitable mission and fiduciary responsibilities and, in turn, to minimize the misuse of charitable assets. Donors, who want assurance that their contributions are safeguarded and used efficiently, may consider information on a nonprofit's governance structure when making funding decisions.

The primary purpose of our paper is to determine whether donors value corporate governance information. All else equal, we predict that better governance is associated with more giving, as donors opt to support organizations that have better monitoring. Whether donors actually use governance information is an empirical question, as donors may not have the incentive or ability to process such information.

Historically, public information on nonprofit governance has been limited. However, a significant shift in mandatory disclosures occurred in 2008 with the addition of a comprehensive list of governance questions on the Internal Revenue Service (IRS) Tax Form 990, which is the primary source of publicly available information on nonprofit organizations. We exploit this newly available data to investigate the simultaneous effects of a broad set of governance mechanisms on donations for a wide cross-section of nonprofit organizations.

From an empirical perspective, corporate governance is a complex construct. In order to capture the multi-dimensional nature of governance, we include 21 disclosures from the Form 990 in an exploratory factor analysis. Described in more detail later, this analysis identifies seven governance factors representing different latent dimensions. We assign a name to each of these seven factors that reflects the underlying Form 990 disclosures with the highest factor loadings. These factors, and an example of a corresponding Form 990 disclosure, include: *Policies* (e.g., whistleblower policy statement), *Audit* (e.g., existence of an audit committee), *Compensation* (e.g., executive compensation approved by independent persons), *Board* (e.g., percentage of the board that is independent), *Management* (e.g., absence of related parties), *Access* (e.g., Form 990 posted on the organization's website), and *Minutes* (e.g., documentation of board meetings).

Next, to assess the extent to which governance is associated with the level of subsequent donations, we estimate a regression of total donations on the seven governance factors and control variables. We find consistent evidence that *Policies, Audit, Compensation, Board, Management,* and *Access* are positively associated with total donations. Organizations appear to receive the greatest benefit from better governance with respect to executive compensation, but each of the governance factors has incremental explanatory power, suggesting that donors view these as complementary governance mechanisms. Our results are robust across various model specifications and are consistent with the notion that donors, particularly, large donors, consider corporate governance when making giving decisions.

Our primary results show that governance information is value-relevant to donors, but they do not address whether the Form 990 has information content in the sense that donors obtain governance information directly from the Form 990. For example, some institutional donors, such as private foundations, corporations, and government agencies, may privately obtain governance information (Hedge, Nico, and Fox 2009). Nevertheless, an IRS Commissioner characterized the new Form 990 governance disclosures as the "crown jewel" of efforts to improve oversight in the nonprofit sector (Miller 2008). Thus, we expect that the revised Form 990 increased awareness and access to governance information. To shed some light on the role of the Form 990 in providing governance information, we conduct a supplemental analysis in which we compare the association between donations and governance before and after the Form 990 revision. Our results generally indicate a stronger association between governance and donations after the Form 990 revision, consistent with the Form 990 disclosures having incremental information content. Moreover, this supplemental test provides corroborating evidence related to our primary research question, which is whether donors value corporate governance.

This study has several implications for nonprofit managers, policy makers, and researchers. As discussed in the next section, few prior studies examine the extent to which donors value various governance mechanisms used in the nonprofit sector. Our findings indicate that underinvestment in the organization's governance structure can have negative consequences on donations received. This evidence suggests that nonprofit managers and boards of directors should not have a myopic focus on efficiency ratios, because donors and government grantors appear to also consider governance attributes. Our evidence may also assist charity-rating agencies, which are refining their standards to better measure an organization's effectiveness.

Our results are also relevant to the ongoing public policy debate in the United States concerning nonprofit regulations. The 2008 changes made to the Form 990 are controversial (Brody 2012), and opponents to the increased disclosure argue that such information is irrelevant to donors.[1] In contrast, our results provide evidence consistent with the governance items on the Form 990 reflecting information used by donors.

The recent IRS reform for nonprofit organizations provides an interesting and useful contrast to governance reforms for publicly traded companies. The IRS has taken a disclosure approach, where nonprofit organizations annually report whatever governance practices are in place. In comparison, the Securities and Exchange Commission (SEC) and the exchanges have taken a mandatory adoption approach, which requires publicly traded companies in the United States to implement specific governance practices. Bhagat, Bolton, and Romano (2008) argue that because good governance is context-specific, a disclosure regime may be preferable to a mandatory adoption regime. Our paper provides initial evidence on the effects of a disclosure regime because decision makers appear to value the information contained in governance disclosures. We document that the nonprofit sector did not universally adopt all governance items on the Form 990, but that more organizations strengthened their governance practices than weakened them in 2009 and 2010, consistent with the idea that required disclosure can induce a particular behavior when the behavior is cost-effective.

Finally, to the best of our knowledge, we are the first researchers to identify a set of governance dimensions from the comprehensive list of questions on the revised Form 990. Similar to Larcker, Richardson, and Tuna (2007) in the for-profit literature, our seven factors are a first step in developing a parsimonious blueprint of the complex construct of corporate governance for nonprofit organizations, which can aid future nonprofit research.

The next section motivates our central hypothesis on the link between governance and donations and discusses related research. Section III describes our sample selection process, while Section IV presents the factor analysis and discusses the governance factors. Section V examines the association between donations and the corporate governance factors. In Section VI, we discuss the limitations of our analysis and conclude.

II. HYPOTHESIS DEVELOPMENT AND RELATED LITERATURE

Agency problems, which traditionally result from the separation of ownership and control, exist in most organizations regardless of the ownership structure (Jensen and Meckling 1976). Nonprofits can suffer agency losses if nonprofit managers shirk their responsibilities, pursue their own agendas, or appropriate excessive compensation and perquisites (Glaeser 2003). Because nonprofit organizations are established to fulfill a charitable mission, they do not distribute profits or operate with clear lines of ownership and accountability (Fama and Jensen 1983). In the absence of owners, donors often assume the role of *de facto* principals in nonprofit principal-agent models (Hansmann 1996).

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[1] For example, when discussing disclosure, governance, and audit reforms at the state level, Mulligan (2007) writes, "It is also doubtful that donors or other members of the public will make use of greater mandatory disclosure of financial data. One assumption underlying these legislative efforts appears to be that donors will use this information when making gifts."

Corporate governance arises out of the need to reduce agency losses. Effective governance limits misuse of charitable assets and better aligns nonprofit managers' personal objectives with those of their organizations and the public they serve. Information on organizations' governance structures can assist donors in assessing the likelihood that their donations will be used in an appropriate and efficient manner. Thus, our central hypothesis is:

> **H:** All else equal, better-governed nonprofit organizations receive more donations.

This hypothesis is based on the assumption that donors make giving decisions to fulfill a charitable mission and, therefore, support organizations they perceive to be most likely to use their donations effectively to support the mission.

In support of this hypothesis, anecdotal evidence suggests that institutional donors (Hedge et al. 2009; Conkley 2006) and wealthy individuals (Winter 2002; Bernstein 2008) consider, and sometimes attempt to improve, a recipient organization's corporate governance. Additionally, nonprofit watchdogs, such as Charity Navigator and Better Business Bureau Wise Giving Alliance, have incorporated governance information in their publicly available ratings of nonprofit organizations. Moreover, the business press (e.g., Stephens and Flaherty 2013) uses the Form 990 to investigate nonprofit governance and leadership issues. Finally, experimental evidence suggests that donors perceive poorly governed nonprofits less favorably (Flynn 2011). These examples demonstrate that donors can obtain governance information from several sources, including the Form 990, private information collection (i.e., grant applications), indirect channels (i.e., business press and watchdogs), and other voluntary disclosures (i.e., websites).

There are also reasons to expect that the quality of corporate governance may not influence donor decisions. First, unlike shareholders, donors often do not directly benefit from a nonprofit organization's activities and, thus, some may have less incentive to monitor the organization (Fama and Jensen 1983). Second, some donors directly observe an organization's charitable performance and, thus, are not concerned with governance. Third, some donors are motivated by other considerations, such as a "warm glow" from the act of giving (Andreoni 1990) or an increase in social status among peers and other private benefits (Glazer and Konrad 1996); such donors are less likely to use governance information in their giving decision. Finally, some donors may not know how to appropriately interpret and use governance information. For these reasons, it is an open empirical question whether the quality of corporate governance is associated with the level of donations.

We are aware of no studies that consider the effect of a broad range of governance mechanisms on donations and only four published studies that examine specific mechanisms. Olson (2000) examines the relationship between board characteristics and donations for 43 independent colleges from 1991 to 1995. He reports a positive association between donations and board size, board tenure, and the number of members with executive backgrounds. Aggarwal, Evans, and Nanda (2012) also report a positive association between board size and donations for a large cross-section of organizations from 1998 to 2003, although they attribute this result to larger boards achieving better board fundraising rather than better board oversight. Kitching (2009) examines whether the level of donations received by a nonprofit is associated with audit quality. Using information on 295 organizations for fiscal years 1995 through 2002, she documents a positive association between a nonprofit's use of a Big 5 auditor and donations received. Finally, Saxton, Neely, and Guo (2014) report a positive relationship between web disclosures, as measures of transparency, and donations for a sample of 400 nonprofits.

Although this prior research makes important initial steps, many questions remain regarding the link between nonprofit governance and donations. Because these studies consider just a single governance dimension, rather than a comprehensive set of dimensions, we do not yet understand how different governance mechanisms used in the nonprofit sector function simultaneously. In addition, the small and narrowly focused samples in three of these studies make it difficult to draw conclusions on how various donors respond to governance.

Our study is the first to investigate the simultaneous effects of a broad set of governance disclosures on donations for a wide cross-section of nonprofit organizations. It is also the first empirical study to examine donor decisions using the newly available Form 990 governance data. The Form 990 revision increased awareness of governance issues across the sector and provides a rich setting in which to investigate how donors value good governance. More broadly, our study builds on prior research that examines the responsiveness of donations to financial performance, as well as the growing literature on corporate governance in the nonprofit sector.[2]

III. SAMPLE

We acquire a custom dataset of 32,425 annual Form 990 filings from 2008 to 2010 from GuideStar USA.[3] The initial dataset includes all organizations available from GuideStar in June 2010 that were required to file the Form 990 and reported direct donations of at least $10,000. We update the dataset to include all subsequent years of data for these organizations that were available from GuideStar through June 2012. We examine the effect of reported governance on *subsequent* donations and, thus, the first firm-year of our study represents the effect of 2008 governance on 2009 donations. This requirement for lagged data results in an initial sample size of 19,070 observations. We remove 3,198 observations for which all model variables are not available. Our final sample consists of 15,872 firm-year observations for 10,846 unique nonprofit organizations. In terms of industry composition, 40 percent of our sample comes from human services, followed by 22 percent from health and hospitals, 12 percent from education, 9 percent from arts and culture, and the remaining 17 percent from various other industries.

IV. GOVERNANCE FACTORS

Factor Analysis

In order to examine the association between governance and donations, we use the answers to 21 governance-related questions reported on the Form 990. We refer to these Form 990 public disclosures as structural measures because they can be explicitly observed. We next describe each structural measure in conjunction with the results of our factor analysis.

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[2] With respect to donations, prior research suggests that donors react to financial information in many, but not all, cases (Gordon et al. 2009). With respect to nonprofit governance, there is limited prior research. Most prior studies suggest that good governance improves nonprofit operating performance and reporting (Callen and Falk 1993; Callen, Klein, and Tinkelman 2003; Desai and Yetman 2006; M. Yetman and R. Yetman 2012).

[3] The advantage of using custom data from GuideStar rather than the IRS Statistics of Income (SOI) data is avoiding the bias toward large firms inherent in the SOI data. Many of the concerns about poor governance relate to small and mid-sized organizations. For comparison purposes, mean (median) assets for our sample is $30.7 million ($6.3 million), while the mean (median) assets for the 2008 SOI file is $155 million ($31.7 million). The mean (median) assets for organizations required to file a revised Form 990 in 2008 from the National Center for Charitable Statistics (NCCS) Core file, which includes all organizations, but does not provide governance data, is $54.7 million ($7.4 million). Thus, the custom data result in a more representative sample of the nonprofit sector than the SOI data.

Table 1 lists each structural measure with a brief description and its line reference on the Form 990, plus the sample mean. Almost all of the disclosure questions yield a response of "yes" or "no," which we convert into an indicator measure so that each measure is increasing in governance quality. Table 1 shows that there is a range in the rate of adoption across different governance measures. For example, 97 percent of organizations keep minutes of board meetings (*doc_gov*), but only 11 percent provide information on their website (*own_web*).

Empirically evaluating the quality of corporate governance is challenging because governance can have multiple underlying dimensions that are not readily measured. Using an individual indicator of governance or a naïve summation of indicators to represent underlying governance dimensions can result in measurement error and inconsistent regression coefficients (Larcker et al. 2007). Factor analysis provides one parsimonious way to represent the latent governance dimensions in the data. Factor analysis extracts the common variance in the observable structural measures in order to identify governance factors, where each factor measures an underlying governance dimension with less measurement error than the observable structural measures. Therefore, we employ principal component factor analysis with promax rotation to identify governance factors and their underlying structures.[4]

There is debate over whether researchers should use binary structural measures in a traditional exploratory factor analysis. Generally, exploratory factor analysis uses a Pearson correlation matrix, which assumes continuous, normally distributed variables. The concern is that including discrete variables will result in factors based on structural measures with similar distributions rather than similar attributes, making the factors less meaningful. Despite this concern, exploratory factor analysis is commonly used with discrete measures because alternative techniques have limitations, as well (Comrey and Levonian 1958; Percy 1976). Comrey and Levonian (1958) argue that the Pearson correlation in traditional factor analysis programs is the most reasonable choice.[5] Thus, we use traditional factor analysis to create one possible representation of the latent nonprofit governance dimensions. We also note that the results from our factor analysis "make sense" in that related structural measures load together in an intuitively reasonable manner.

Governance Factors

Table 2, Panel A presents the results of our factor analysis. We identify seven factors with eigenvalues greater than 1, which together explain over 69 percent of the variance in the original data. We interpret and assign names to the factors by examining the structural measures that have a substantive association with them, which we evaluate as a factor loading

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[4] An alternative data reduction technique is principal component analysis (PCA). PCA is typically used to reduce a set of correlated observed variables into a smaller set of uncorrelated principal components. PCA retains the maximum amount of variance from the original data. Because we want the common variance from our observed measures in order to identify governance dimensions with the least measurement error, we use factor analysis instead of PCA. Additionally, we use exploratory rather than confirmatory factor analysis because we want to identify the underlying factor structure without imposing any preconceived structure on nonprofit governance. Promax oblique rotation is preferred over varimax orthogonal rotation in situations where the outcome factors are believed to be correlated. Our inferences do not change if we rotate our factor matrix using varimax rotation.

[5] Alternative correlation methods include the tetrachoric and polychoric methods, which create normally distributed factors from binary and ordered-category variables. Researchers have compared the various correlation methods and conclude that the choice between them is not crucial (Comrey and Levonian 1958; Percy 1976). We attempt to compute polychoric correlations and find a non-positive definite matrix, which is likely due to the large number of structural variables. When we reduce the number of structural variables to achieve positive definite matrices, our results are similar to those in our main analysis.

TABLE 1

Form 990 Governance Measures
(n = 15,872)

Structural Measure	Description	2008 Form 990 Reference	Coding	Mean
board_size	Number of voting members in the governing body	Part VI Section A Question 1a	Continuous variable	20.60
independence	Percentage of independent voting members to total board members	Part VI Section A Question 1b	Continuous variable	0.90
no_relations	Family/business relationship between key personnel	Part VI Section A Question 2	= 1 for No = 0 for Yes	0.83
no_outsource	Management function delegated to an outside entity	Part VI Section A Question 3	= 1 for No = 0 for Yes	0.96
no_doc_changes	Significant organizational document changes	Part VI Section A Question 4	= 1 for No = 0 for Yes	0.95
no_asset_diversion	Material diversion of assets	Part VI Section A Question 5	= 1 for No = 0 for Yes	1.00
doc_gov	Documentation of governing body meetings	Part VI Section A Question 8a	= 1 for Yes = 0 for No	0.97
doc_comm	Documentation of authorized committees meetings	Part VI Section A Question 8b	= 1 for Yes = 0 for No	0.89
review_990	Governing body review of Form 990 pre-filing	Part VI Section A Question 10	= 1 for Yes = 0 for No	0.81
no_unreachable	Key organization personnel reachable at org address	Part VI Section A Question 11	= 1 for No = 0 for Yes	0.97
conflict	Organization has a written conflict of interest policy	Part VI Section B Question 12a	= 1 for Yes = 0 for No	0.84
officers_conflicts	Key organization personnel disclose conflicts of interest	Part VI Section B Question 12b	= 1 for Yes = 0 for No	0.77
enforce_conflict	Compliance enforcement of conflict of interest policy	Part VI Section B Question 12c	= 1 for Yes = 0 for No	0.74
whistleblower	Organization has written whistleblower policy	Part VI Section B Question 13	= 1 for Yes = 0 for No	0.66
destruction	Organization has written document retention and destruction policy	Part VI Section B Question 14	= 1 for Yes = 0 for No	0.70

(continued on next page)

TABLE 1 (continued)

Structural Measure	Description	2008 Form 990 Reference	Coding	Mean
ceo_salary	Top executive compensation approved	Part VI Section B Question 15a	= 1 for Yes = 0 for No	0.75
officer_salary	Other key personnel compensation approved	Part VI Section B Question 15b	= 1 for Yes = 0 for No	0.58
own_web	Applicable forms disclosed on own website	Part VI Section C Question 18a	= 1 for Yes = 0 for No	0.11
contact_info	Contact information for person with organization records is available	Part VI Section C Question 20	= 1 for No = 0 for Yes	0.95
review_or_audit	Organization's financial statements were either reviewed or audited by an independent accountant	Part XI Questions 2a and 2b	= 1 for Yes = 0 for No	0.86
audit_committee	Organization has an audit committee	Part XI Question 2c	= 1 for Yes = 0 for No	0.76

TABLE 2

Nonprofit Corporate Governance Factor Analysis

Panel A: Rotated Factor Loadings

Structural Measures	*Policies*	*Audit*	*Compensation*	*Board*	*Management*	*Access*	*Minutes*
conflict	**0.874**	0.002	−0.071	0.035	0.000	−0.013	0.008
officers_conflicts	**0.912**	−0.026	−0.037	0.002	−0.017	−0.007	−0.013
enforce_conflict	**0.874**	−0.020	0.012	−0.003	−0.032	−0.010	−0.017
whistleblower	**0.707**	0.002	0.104	0.032	0.014	0.058	−0.021
destruction	**0.689**	−0.002	0.098	0.013	0.010	0.033	−0.009
review_or_audit	−0.067	**0.951**	−0.033	0.005	−0.002	−0.029	−0.044
audit_committee	0.033	**0.895**	0.023	0.035	0.000	0.031	−0.024
ceo_salary	0.106	0.041	**0.822**	−0.008	0.060	−0.020	−0.014
officer_salary	−0.038	−0.042	**0.938**	−0.023	0.034	−0.028	−0.060
ln board_size	0.026	0.109	0.056	**0.645**	0.057	0.173	0.112
independence	0.044	−0.026	−0.066	**0.787**	−0.052	−0.198	−0.022
no_relations	0.091	0.096	−0.145	−0.183	**0.485**	0.006	−0.015
no_outsource	−0.137	−0.017	0.219	0.052	**0.592**	0.139	0.050
no_doc_changes	−0.005	−0.051	0.037	−0.006	**0.537**	−0.327	−0.010
no_unreachable	0.081	−0.007	−0.079	0.061	**0.475**	0.013	0.046
own_web	0.028	0.018	−0.016	−0.088	−0.048	**0.817**	0.111
contact_info	−0.038	−0.103	−0.101	0.030	0.096	**0.493**	−0.368
doc_comm	−0.034	−0.059	−0.029	0.061	0.091	0.115	**0.844**
doc_gov	−0.013	−0.023	−0.062	−0.006	−0.026	−0.017	**0.868**
review_990	0.138	0.080	0.136	−0.213	−0.105	0.042	0.198
no_asset_diversion	0.063	0.050	−0.112	−0.096	0.170	−0.052	0.037

The structural measures are defined in Table 1. We use principal component factor analysis with promax rotation and identify factors with eigenvalues greater than 1. The bold values represent factor loadings greater than 0.4.

Panel B: Factor Descriptive Statistics (n = 15,872)

Factor	**Variance Explained**	**Q1**	**Mean**	**Median**	**Q3**	**Std. Dev.**
Policies	19.24%	−0.303	0.000	0.609	0.717	1.000
Audit	10.36%	0.085	0.000	0.499	0.576	1.000
Compensation	12.48%	−0.685	0.000	0.553	0.798	1.000
Board	5.53%	−0.417	0.000	−0.036	0.364	1.000
Management	5.70%	−0.227	0.000	0.398	0.473	1.000
Access	5.61%	−0.508	0.000	−0.319	−0.080	1.000
Minutes	10.11%	0.065	0.000	0.339	0.394	1.000

See Panel A for the structure of each factor.

greater than or equal to 0.4. Our factors are *Policies, Audit, Compensation, Board, Management, Access,* and *Minutes,* and they are all increasing in "good" governance. Table 2, Panel B reports the descriptive statistics for the factors and shows that *Policies* explains most of the variance in the original data, followed by *Compensation, Audit, Minutes, Management, Access,* and *Board.*

We next discuss each factor by presenting the underlying substantive structural measures and their relation to governance.[6] We also provide theory for each factor's expected effect on giving, but acknowledge that we use the word loosely as there is little formal theory for selecting the appropriate governance characteristics that relate to giving.

Policies

Policies is increasing in the existence, monitoring, and enforcement of a formal written conflict of interest policy (*conflict, officers_conflicts, enforce_conflict*), in the existence of a whistleblower policy (*whistleblower*), and in the existence of a document destruction policy (*destruction*). The Form 990 questions concerning these policies reflect the recommendations for good governance developed by the Panel on the Nonprofit Sector (2007). Key individuals owe their respective charities a duty of loyalty that requires them to avoid interactions that are detrimental to the charity; a conflict of interest policy can identify and reduce such problems. Additionally, an effective policy for employees to confidentially report unethical behavior can encourage such reporting and help protect charitable resources. An effective document policy can help ensure that relevant documents are available to protect an organization's exempt status, as well as to evaluate the success of its programs, monitor its budget, and prepare its financial statements and returns. These policies are intended to encourage key individuals to engage in activities that benefit the interest of the nonprofit and can increase donor confidence that their donations are being used to further the mission of the nonprofit. To the extent that these policies enhance corporate governance, we expect a positive association between *Policies* and donations.

Audit

Audit is increasing in the review or audit of the organization's financial statements by an independent accountant (*review_or_audit*) and in the presence of a committee that oversees the financial reporting process (*audit_committee*). While there are significant differences between the objectives of a review and an audit, they both involve the use of an independent accountant.[7] Using an independent accountant in the financial reporting process provides external oversight that can mitigate agency costs. The Panel on the Nonprofit Sector (2007) recommends that organizations have an independent audit committee to provide additional assurance that the audit is conducted appropriately and the organization is producing high-quality information. The IRS (2008) suggests that a charity with substantial assets or revenues should consider contracting for an audit, and argues that the use of an audit committee can help ensure that the external accountant is competent and independent and that the overall quality of the audit is acceptable.

The structural measures underlying *Audit* suggest that *Audit* enhances internal controls and financial reporting quality. This increase in accountability will give donors more assurance that the organization's information is reliable and charitable assets are safeguarded. All else equal, the improvement in donor confidence should lead to more donations; therefore, we expect a positive association between *Audit* and donations.

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[6] Two of our structural measures, *review_990* and *no_asset_diversion*, do not substantively load with any particular factor; *review_990* reflects whether an organization provides a copy of its Form 990 to the board before the form is filed with the IRS, and *no_asset_diversion* reports whether the organization became aware during the year of a material diversion of its assets (e.g., theft). While neither measure substantively loads with a particular factor, they do both contribute in small part to each of the seven factors, as seen in Table 2.

[7] We consider review and audit together rather than separately in our factor analysis because they both relate to financial reporting oversight, but there is an inherent negative correlation between them, which results from the fact that a nonprofit will not undergo both a review and an audit. In our sample, 82 percent of organizations undergo an audit, while only 4 percent undergo a review. Our factors and primary results are qualitatively similar if we use an audit structural variable that excludes reviews.

Compensation

Compensation is increasing in the application of specific criteria to determine the compensation of the CEO (*ceo_salary*) and other key employees (*officer_salary*), such as a review and approval by independent persons, use of comparability data, and contemporaneous substantiation of the deliberation and decision. The IRS (2008) argues that implementing these policies will help ensure that reasonable compensation is paid and help reduce agency problems, where managers seek to maximize their own perquisites at the expense of the nonprofit's public stakeholders. Balsam and Harris (2014) show that donations from sophisticated donors are negatively associated with compensation *levels*, whereas we examine compensation *policies* that can help to assure donors that their gifts are not being used to fund excessive salaries and perquisites. We expect a positive association between *Compensation* and donations.

Board

Board is increasing in board size (natural log of *board_size*) and board independence (*independence*). Nonprofits must provide board of directors information, including the total number of members and the number of independent voting board members. Jensen (1993) describes the board of directors as the apex of the governance system and as having ultimate responsibility for the effective functioning of the organization. Consistent with this description, the IRS (2008) notes that an effective board is important to an organization's success and its compliance with tax laws. The Panel on the Nonprofit Sector (2007) recommends that a substantial majority of the board should be independent because independent members are less conflicted, more attentive to the nonprofit mission, and less likely to self-deal. With respect to size, larger boards are more likely to possess specific knowledge and expertise. To the extent that donors feel that board oversight helps ensure that their donations are used in an efficient manner toward the charitable purpose, we expect a positive association between *Board* and donations.[8]

Management

Management is increasing in the absence of certain management-related weaknesses. These include the absence of related parties (*no_relations*), the absence of outsourcing of managerial duties normally performed by key individuals (*no_outsource*), the absence of key persons being unreachable at the organization's address (*no_unreachable*), and the absence of significant changes being made to organizational documents since the prior Form 990 (*no_doc_changes*). The IRS (2008) argues that family and/or business relations between key personnel impair independence, which can compromise the nonprofit's ability to represent broad public interest or result in insider transactions, which squander charitable assets. Outsourcing managerial duties increases the separation between donors and management in the nonprofit sector, which amplifies agency problems by making it more difficult to ensure that work is being performed accurately or that the appropriate amount of effort is being expended (Cannon and Growe 2005; Geis 2007). A similar argument can be made if key persons are not reachable at the organization's address. Finally, while significant changes to organizational documents (i.e., articles of incorporation, bylaws) since the prior Form 990 was filed are not inherently all good or all bad, any change could jeopardize an organization's tax-exempt status if the changes are inconsistent with the organization's mission. Overall, the absence of these management-related weaknesses increases the likelihood of the organization's resources being used appropriately. Thus, we expect a positive association between *Management* and donations.

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[8] In our primary analysis, we assume that more governance is better. We recognize that this assumption might not always hold, especially with respect to board size, which is included in *Board*. Directors on very large boards may be detached and less effective monitors. We address a potentially nonlinear relationship between board size and good governance in a supplemental analysis.

Access

Access addresses transparency and is increasing in the availability of the organization's tax documents via its own website *(own_web)* and the reporting on the Form 990 of the contact information for the person responsible for the organization's records (*contact_info*). The Panel on the Nonprofit Sector (2007) recommends that organizations make financial information, as well as information on the board and staff members, widely available. Donors are more likely to obtain information directly from the nonprofit itself than from other sources (Cnaan, Jones, Dickin, and Salomon 2011). Better access helps donors ensure that their donations are used efficiently and, thus, we expect a positive association between *Access* and donations.

Minutes

Minutes reflects the documentation of the meetings of the board (*doc_gov*) and the meetings of each committee with authority to act on behalf of the board (*doc_comm*). The IRS (2008) encourages boards and committees to prepare meeting minutes. Maintaining such records not only complies with laws, but can also help protect against allegations of wrongdoing (Panel on the Nonprofit Sector 2007). Donors may prefer that an organization maintains business records because this practice reflects a more conscientious deliberation process, implying a positive expected association between *Minutes* and donations.

V. EMPIRICAL ANALYSIS

Methodology

We next examine the association between the seven governance factors and total donations received in the subsequent period. Our empirical model builds on the donor's demand model that was first developed by Weisbrod and Dominguez (1986) and that has been used extensively in both economics and accounting academic research. We add our governance factors to the model and estimate the following equation:

$$\begin{aligned}\ln TOTAL\ DONATIONS_{i,t} = {} & \beta_0 + \beta_{1-7} GOV\ FACTORS_{i,t-1} + \beta_8 PROGRAM\ RATIO_{i,t-1} \\ & + \beta_9 \ln FUNDRAISING\ EXP_{i,t-1} + \beta_{10} \ln TOTAL\ ASSETS_{i,t-1} \\ & + \beta_{11} \ln PROGRAM\ SERVICE\ REV_{i,t-1} + \beta_{12} \ln AGE_{i,t-1} \\ & + \sum \gamma_i INDUSTRY_i + \Sigma \delta_i YEAR_t + \varepsilon_i. \qquad (1)\end{aligned}$$

TOTAL DONATIONS includes donations from individuals, corporations, and foundations, as well as government grants and indirect support from federated campaigns. Our primary variables of interest are *GOV FACTORS*, which represent the seven governance factors from our factor analysis (*Policies, Audit, Compensation, Board, Management, Access, Minutes*). We expect positive coefficients on *GOV FACTORS*, consistent with our overall hypothesis that higher-quality governance is associated with more donations.[9]

PROGRAM RATIO equals the ratio of program expenses to total expenses. It is intended to control for the efficiency with which an organization supports its mission and should be positively associated with donations. *FUNDRAISING EXP* is the reported dollar amount of fundraising expenses

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[9] We use adjusted standard errors clustered by organization (White 1980; Peterson 2009) and remove influential observations using studentized residuals greater than the absolute value of three (Belsley, Kuh, and Welsch 1980).

TABLE 3

Donor's Demand Model Descriptive Statistics

Variable (n = 15,872)	Q1	Mean	Median	Q3	Std. Dev.
TOTAL DONATIONS	318,018	4,304,941	959,707	2,856,185	23,500,000
PROGRAM RATIO	0.762	0.813	0.839	0.898	0.132
FUNDRAISING EXP	0	250,060	42,315	183,103	1,684,247
TOTAL ASSETS	3,828,652	30,700,000	6,308,488	14,300,000	248,000,000
PROGRAM SERVICE REV	0	15,900,000	0	978,658	389,000,000
AGE	17	39	31	51	33
DIRECT DONATIONS	179,621	2,727,117	563,131	1,636,916	15,800,000
GOV GRANTS	0	1,432,876	0	358,895	13,900,000
FEDERATED CAMPAIGNS	0	40,038	0	0	750,461
ZERO FUND	0.000	0.307	0.000	1.000	0.461
SINGLE AUDIT	0.000	0.165	0.000	0.000	0.371
INT CONTROL PROBLEM	0.000	0.037	0.000	0.000	0.188

IRS Form 990 references are: *TOTAL DONATIONS* (Part VIII, 1h); *FUNDRAISING EXP* (Part IX, 25D); *TOTAL ASSETS* (Part X, 16B); *PROGRAM SERVICE REV* (Part VIII, 2g); *DIRECT DONATIONS* (Part VIII, 1f); *GOV GRANTS* (Part VIII, 1e); and *FEDERATED CAMPAIGNS* (Part VIII, 1a). *PROGRAM RATIO* equals program expenses (Part IX, 25B) divided by total expenses (Part IX, 25A). *AGE* is calculated from the year of initial 501(c)(3) filing for tax-exempt status from the NCCS Business Master File. Raw values are presented for illustrative purposes only; in the multivariate models, we use the natural log of certain variables as indicated in subsequent tables.

Variable Definitions:
ZERO FUND = an indicator variable set to 1 if the organization reports zero fundraising expenses;
SINGLE AUDIT = an indicator variable set to 1 if the organization filed a single audit report with the Federal Audit Clearinghouse; and
INT CONTROL PROBLEM = an indicator variable set to 1 if the organization had a reportable condition over financial reporting in its single audit report.

and is included as a proxy for fundraising effort, which is necessary because nonprofits engage in fundraising with the intent of raising donations.[10] *TOTAL ASSETS* controls for scale effects. *PROGRAM SERVICE REV* represents the other main source of nonprofit revenue and controls for any crowding-out or crowding-in effects. *AGE* is the number of years since the organization applied for tax-exempt status. Okten and Weisbrod (2000) suggest that age can serve as a proxy for reputation and be positively associated with donations. Alternatively, age can serve as a proxy for need and be negatively associated with donations as older organizations tend to have accumulated reserves. We also include industry and year controls. Table 3 reports descriptive statistics for these variables.

Primary Analyses

Main Results

Table 4, Panel A reports the results from estimating our model using one of the seven governance factors in each of the seven columns. The coefficients on all seven factors are significantly positive,

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[10] Krishnan, M. Yetman, and R. Yetman (2006) find that some nonprofits report exactly zero fundraising expense when they plausibly should be reporting nonzero amounts, which introduces measurement error into our fundraising effort proxy. We take two approaches to address this potential measurement error. First, following M. Yetman and R. Yetman (2013), we compute an *estimated* amount of fundraising expense for those organizations that reported zero fundraising. Second, we eliminate organizations that report zero fundraising. Untabulated results from these alternative specifications show that all variables of interest remain significant at conventional levels, and all inferences remain the same.

TABLE 4

The Effect of Corporate Governance on Donations

Panel A: Individual Governance Factor Regressions

	Dependent Variable: ln *TOTAL DONATIONS*						
	Policies	*Audit*	*Compensation*	*Board*	*Management*	*Access*	*Minutes*
GOV FACTORS (defined in column heading)	0.217***	0.166***	0.226***	0.217***	0.103***	0.217***	0.090***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
PROGRAM RATIO	1.394***	1.458***	1.393***	1.467***	1.436***	1.408***	1.445***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
ln *FUNDRAISING EXP*	0.099***	0.101***	0.098***	0.100***	0.106***	0.099***	0.104***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
ln *TOTAL ASSETS*	0.474***	0.501***	0.479***	0.492***	0.520***	0.490***	0.500***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
ln *PROGRAM SERVICE REV*	−0.031***	−0.029***	−0.032***	−0.029***	−0.029***	−0.028***	−0.030***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
ln *AGE*	−0.027*	−0.033**	−0.032**	−0.048***	−0.028*	−0.027*	−0.023
	0.065	0.025	0.028	0.002	0.058	0.063	0.127
Constant	4.086***	4.221***	4.156***	4.433***	3.234***	3.829***	4.171***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Industry and Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	15,751	15,755	15,754	15,754	15,761	15,752	15,757
Adjusted R^2	0.2689	0.2630	0.2699	0.2625	0.2577	0.2696	0.2576

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*, **, *** Represent significance levels of 10 percent, 5 percent, and 1 percent, respectively (two-tailed).

The governance factors are detailed in Table 2. The other variables are defined in Table 3, with the exception of *INDUSTRY*, which represents the NTEE 12 classifications. The dependent variable is measured at time *t*, and the independent variables are measured at time *t*–1. Coefficients and their respective p-values are presented. We use adjusted standard errors clustered by organizations and remove influential observations using studentized residuals greater than the absolute value of 3.

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TABLE 4 (continued)

Panel B: Combined Governance Factor Regressions

	Dependent Variable: ln *TOTAL DONATIONS*					
	Base	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Policies		0.133***	0.021***	0.019***	0.017***	0.022***
		0.000	0.000	0.001	0.004	0.000
Audit		0.104***	0.016***	0.015**	0.015**	0.020***
		0.000	0.005	0.010	0.011	0.001
Compensation		0.137***	0.028***	0.027***	0.026***	0.028***
		0.000	0.000	0.000	0.000	0.000
Board		0.150***	0.007**	0.006*	0.010**	0.011*
		0.000	0.041	0.077	0.022	0.089
Management		0.104***	0.020***	0.021***	0.018***	0.019***
		0.000	0.000	0.000	0.001	0.000
Access		0.176***	0.025***	0.022***	0.024***	0.024***
		0.000	0.000	0.000	0.000	0.000
Minutes		0.025*	0.003	0.004	0.001	0.002
		0.068	0.596	0.469	0.784	0.720
PROGRAM RATIO	1.452***	1.351***	0.114**	0.269***	0.078*	0.111**
	0.000	0.000	0.011	0.000	0.083	0.014
ln *FUNDRAISING EXP*	0.106***	0.083***	0.005***	0.035***	0.008***	0.005***
	0.000	0.000	0.000	0.000	0.000	0.000
ln *TOTAL ASSETS*	0.508***	0.455***	0.019***	0.011*	0.023***	0.018***
	0.000	0.000	0.001	0.076	0.000	0.002
ln *PROGRAM SERVICE REV*	−0.029***	−0.031***	−0.002**	−0.002**	−0.003***	−0.002*
	0.000	0.000	0.040	0.017	0.005	0.075
ln *AGE*	−0.019	−0.083***	0.003	0.002	−0.003	0.002
	0.211	0.000	0.589	0.747	0.610	0.647
ln *TOTAL DONATIONS*			0.925***	0.916***	0.904***	0.921***
			0.000	0.000	0.000	0.000
ZERO FUND				0.623***		
				0.000		

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TABLE 4 (continued)

	Dependent Variable: ln *TOTAL DONATIONS*					
	Base	(1)	(2)	(3)	(4)	(5)
*ZERO FUND * PROGRAM RATIO*				−0.330***		
				0.001		
SINGLE AUDIT					0.219***	
					0.000	
INT CONTROL PROBLEM					−0.028	
					0.215	
Constant	3.810***	5.542***	0.681***	0.474***	0.913***	0.619***
	0.000	0.000	0.000	0.000	0.000	0.000
Industry and Year	Yes	Yes	Yes	Yes	Yes	Yes
State						Yes
n	15,755	15,755	15,548	15,548	15,546	15,492
Adjusted R^2	0.2549	0.3003	0.8697	0.8700	0.8711	0.8683

*, **, *** Represent significance levels of 10 percent, 5 percent, and 1 percent, respectively (two-tailed).

The governance factors are detailed in Table 2. The other variables are defined in Table 3. The dependent variable is measured at time *t*, and the independent variables are measured at time *t*–1. Coefficients and their respective p-values are presented. We use adjusted standard errors clustered by organizations and remove influential observations using studentized residuals greater than the absolute value of 3.

consistent with our predictions. Panel B, column (1) reports the results of Equation (1) with all seven factors included simultaneously. Again, we find that *Policies, Audit, Compensation, Board, Management, Access,* and *Minutes* are significantly associated with total donations, suggesting that donors view each of the governance factors as important in its own right. In order to assess the total explanatory power of these factors, we present the results from estimating the standard donor's demand model without the factors as a benchmark in the Base column. The adjusted R^2 increases by 0.0454 from 0.2549 to 0.3003 when the governance factors are included.

Endogeneity

Endogeneity is a common concern in governance studies. In our setting, it is possible that donations and governance are simultaneously determined or that the exogenous determinants of an organization's governance policies also influence the amount of donations received. We address endogeneity in four ways: (1) including lagged donations; (2) adding additional variables that may be correlated with both donations and governance; (3) measuring governance using the deviation from predicted governance; and (4) comparing the link between donations and governance before and after the Form 990 revision.[11] We discuss the first three approaches here and the fourth approach in the "Supplemental Analyses" subsection. Because each approach has limitations, we base our conclusions on the collective evidence rather than any single approach.

Inclusion of the lagged dependent variable. If donations and governance are simultaneously determined, then including lagged donations should reduce the observed association between current donations and lagged governance factors. In addition, including lagged donations helps rule out concerns about omitted correlated variables because lagged donations control for time-invariant organizational characteristics, such as fundraising aptitude. In order for an omitted variable to cause the observed association between donations and a specific governance factor, that omitted variable must be correlated with $TOTAL\ DONATIONS_t$ and the specific $GOV\ FACTORS_{t-1}$, but not correlated with $TOTAL\ DONATIONS_{t-1}$ or the six other $GOV\ FACTORS_{t-1}$.

Column (2) of Table 4, Panel B shows the results when including $TOTAL\ DONATIONS_{t-1}$ in our model. The coefficients on each of the governance factors are lower in magnitude. However, consistent with column (1), we continue to find that *Policies, Audit, Compensation, Management,* and *Access* are significantly associated with donations at the $p < 0.01$ level, and *Board* is significant at the $p < 0.05$ level. The coefficient on *Minutes* is not significant when we control for $TOTAL\ DONATIONS_{t-1}$. We retain the lagged dependent variable for all subsequent tests in Table 4, but our results are robust to excluding it.[12]

Results in column (2) of Table 4, Panel B suggest that moving from the 25th to the 75th percentile of *Policies, Audit, Compensation, Board, Management, Access,* and *Minutes* is associated with 2.17 percent, 0.79 percent, 4.24 percent, 0.55 percent, 1.41 percent, 1.08 percent, and 0.10

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[11] Instrumental variable estimation is the textbook solution when omitted variable, simultaneity, or reverse causation issues exist. Using instrumental variables in this setting is not practical because we have seven governance variables and would need to identify seven separate instruments. Each of these instruments would need to be correlated with a separate governance factor, but not correlated with the error term in the original donations model. Larcker and Rusticus (2010) show that if we use "bad" instruments that are only weakly correlated with governance and partially correlated with the error term, then instrumental variable estimates will be more biased than OLS estimates. Because we are unable to identify seven good instruments, we do not use instrumental variable estimation.

[12] One particular concern is the possibility that management effectiveness is an omitted variable, influencing both governance and donations. We identify a subsample of organizations that do not experience a manager turnover, as determined by the name of the principal officer on the Form 990. $TOTAL\ DONATIONS_{t-1}$ is a particularly strong control for management's effectiveness at extracting donations in this subsample because the same principal officer is fundraising in both years. When we reestimate our model for this subsample, in untabulated results, the coefficients on *Policies, Audit, Compensation, Management,* and *Access* are significantly positive, consistent with Table 4, column (2). The coefficient on *Board* is positive, but not significant in a two-tailed test ($p = 0.189$).

percent more donations, respectively, or 10.34 percent more in aggregate.[13] As a benchmark to gauge relative economic significance, prior research documents the association between nonprofit efficiency and donations. Our evidence indicates moving from the 25th to the 75th percentile of the program expense ratio, a measure of efficiency, results in 1.56 percent more donations. As an alternative benchmark, Petrovits, Shakespeare, and Shih (2011) use a similar model and report that organizations with internal control problems receive 3.8 percent less public support.

Inclusion of additional control variables. We further reduce the risk of omitted variables by including other potentially confounding variables. First, we add controls for financial reporting quality. Prior research suggests that donors reward organizations with higher financial reporting quality and that organizations with better governance have higher financial reporting quality (Yetman and Yetman 2012, 2013). Thus, the observed association between total donations and governance may be due to an omitted correlated variable, financial reporting quality. To address this concern, we control for reporting quality with *ZERO FUND,* which is equal to 1 if the organization reports no fundraising expenses on its Form 990, and 0 otherwise, and interact it with the program ratio, which is the primary financial metric in our model. Results reported in column (3) of Table 4, Panel B show that the coefficients on *Policies, Audit, Compensation, Board, Management,* and *Access* remain positive and statistically significant.[14]

Second, we add controls for single audits and internal control problems. Organizations that receive substantial federal funding are required to undergo a single audit whereby independent auditors issue opinions on the organization's financial statements, internal controls, and compliance with government regulations. It is possible that the requirement to undergo a single audit is associated with our governance factors, and donors respond to the increased monitoring from a single audit rather than governance information. Additionally, poor governance may be associated with internal controls problems, which are associated with decreased donations (Petrovits et al. 2011). We obtain single audit data for our sample from the Federal Audit Clearinghouse and include indicator variables for whether an organization had a single audit (*SINGLE AUDIT*) and, if so, whether any reportable conditions over financial reporting (*INT CONTROL PROBLEM*) were noted. The results in Table 4, Panel B, column (4) indicate that having a single audit is associated with more donations.[15] Nevertheless, the coefficients on *Policies, Audit, Compensation, Board, Management,* and *Access* are still significant.

Finally, we add controls for state regulation. It is possible that our governance factors are correlated with mandated governance policies, which vary from state to state. As such, our results may be driven by differences in giving across states rather than differences in governance across organizations. To control for this possibility, we include state fixed effects. As presented in column (5) of Table 4, Panel B, *Policies, Audit, Compensation, Board, Management,* and *Access* remain significantly associated with total donations.

Deviations from predicted level of governance. This approach uses an alternative measure of governance that represents deviations from the cross-sectional average level of governance. Larcker et al. (2007) argue that deviations from the average governance structure can be used when examining the association between governance and economic outcomes. This approach assumes that organizations, on average, have optimal governance structures, which represent the systematic or endogenous part of an organization's governance structure. Thus, deviations from the average structure are less plagued with endogeneity concerns. They measure deviations as cross-sectional differences in governance by size and industry. We take a similar approach by first regressing each of our governance factors on size (total assets) and industry (National Taxonomy of Exempt Entities [NTEE] 12 industries). The residuals from these regressions represent governance deviations, which we then include as *GOV FACTORS* in our main model. Our untabulated results are consistent with the results in column (1) of Table 4, Panel B. In total, these analyses provide some comfort that our primary results are not dominated by endogeneity effects.

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[13] To estimate the economic significance for each factor, we (1) compute the difference between the third and first quartile from Table 5, Panel B; (2) multiply that difference by the regression coefficient; (3) compute the inverse natural log of that product because the dependent variable is logged donations; and (4) subtract 1. For instance, the calculations for *Audit* are as follows: (1) $0.576 - 0.085 = 0.491$; (2) $0.491 * 0.016 = 0.0079$; (3) $\exp(0.0079) = 1.0079$; and (4) $1.0079 - 1.0000 = 0.0079 = 0.79$ percent.

[14] Consistent with Yetman and Yetman (2013), the coefficient on *ZERO FUND* is positive because it represents fundraising incurred, which increases donations, but not reported because it is not reflected in *FUNDRAISING EXP*. The interaction is negative because donors rely less on the program ratio when they suspect accounting quality is low.

[15] The coefficient on internal control problems is negative as expected, but not significant ($p = 0.215$). The lack of significance is likely due to the low number of observations that disclosed a reportable condition (3.7 percent of our sample). The sample in Petrovits et al. (2011) consisted only of organizations that received a single audit, resulting in more observations with reportable conditions (14.9 percent of their sample).

Supplemental Analyses

Types of Donors

Total donations are comprised of direct donations, government grants, and federated campaign receipts. To further our understanding of the effect of governance on giving, we separately examine the effect of governance on funding from these various sources.

Direct donations. We examine direct donations first. Evidence in the first column of Table 5 indicates that *Policies, Audit, Compensation, Board, Management,* and *Access* are significantly associated with direct donations, which is consistent with the results using total donations as the dependent variable. We further investigate the effect of governance on direct donor giving by considering differences in donor ability and/or incentives to process governance information. We expect that donors who provide large amounts of funding are more likely to consider governance than are donors who give small amounts. While we are unable to identify the dollar amount of donations that originate from large donors, we are able to identify organizations that receive at least one gift over $5,000 from a single donor using the Form 990, Part IV (2), Schedule B checkbox. For our sample, 85.9 percent of organizations have at least one large donor. We set *LARGE DONORS* to 1 if the organization reports having any large donors in the reporting year and estimate a regression of this variable on our governance factors and controls variables.

Column (2) of Table 5 reports that *Policies, Audit, Compensation, Board,* and *Minutes* are significantly associated with the probability of receiving funds from large donors. The coefficient on *Management* is insignificant. The coefficient on *Access* is significantly negative, suggesting organizations with no large donors attempt to attract funding through public disclosure, such as providing the Form 990 on the organization's website, more so than organizations with large donors. Overall, this evidence generally supports our expectation that large donors are more likely to consider governance than are small donors.

Government grants and federated campaigns. As reported in Table 3, both the median of government grants and the median of federated campaigns equal zero, as the majority of organizations in our sample receive neither source of funding. Good governance may qualify an organization to receive these types of funds. For example, perhaps organizations are required to undergo an audit in order to receive any government grants or federated campaign money. As a result, we use two types of response variables, the level of the funding and an indicator denoting whether the organization receives any funding from that source.[16]

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[16] We restrict our tests on the level of government grants (federated fundraising receipts) to organizations that received at least $1,000 in government grants (federated campaign receipts). We use a probit model to estimate the probability of receiving government grants (federated campaign receipts). We use all observations in our sample when estimating the probit model.

TABLE 5

The Effect of Corporate Governance on Donations by Type of Donor

Dependent Variable	ln *DIRECT DONATIONS* (1)	If *LARGE DONORS* (2)	ln *GOV GRANTS* (3)	If *GOV GRANTS* > 0 (4)	ln *FEDERATED CAMPAIGNS* (5)	If *FEDERATED CAMPAIGNS* > 0 (6)
Policies	0.021***	0.128***	0.124***	0.058**	−0.080	0.303***
	0.002	0.000	0.000	0.014	0.143	0.000
Audit	0.012*	0.078***	0.085***	0.138***	−0.053	0.163***
	0.073	0.008	0.000	0.000	0.213	0.000
Compensation	0.019***	0.077**	0.070***	0.139***	0.083*	0.038
	0.004	0.027	0.005	0.000	0.087	0.298
Board	0.016**	0.099*	−0.020	0.071***	0.017	0.301***
	0.021	0.087	0.272	0.009	0.198	0.000
Management	0.015**	−0.050	0.076***	0.116***	0.067*	−0.007
	0.011	0.138	0.001	0.000	0.078	0.827
Access	0.029***	−0.153***	0.034*	0.067***	0.090***	0.067**
	0.000	0.000	0.086	0.001	0.004	0.019
Minutes	0.004	0.122***	−0.009	−0.006	0.002	0.080*
	0.468	0.000	0.656	0.775	0.968	0.050
PROGRAM RATIO	0.008	−1.214***	1.863***	1.685***	0.827**	1.656***
	0.873	0.000	0.000	0.000	0.029	0.000
ln *FUNDRAISING EXP*	0.013***	0.064***	−0.013***	0.008*	0.041***	0.089***
	0.000	0.000	0.006	0.078	0.000	0.000
ln *TOTAL ASSETS*	0.057***	0.143***	0.316***	−0.085***	0.047	−0.252***
	0.000	0.000	0.000	0.000	0.323	0.000
ln *PROGRAM SERVICE REV*	−0.003***	0.006	−0.016***	0.009***	−0.003	0.007
	0.002	0.285	0.000	0.009	0.644	0.159
ln *AGE*	−0.009	−0.071**	−0.016	0.035*	0.195***	0.493***
	0.129	0.034	0.485	0.091	0.000	0.000
ln *GOV GRANTS*	0.001*	−0.029***	0.285***		0.026***	0.040***
	0.091	0.000	0.000		0.000	0.000
ln *FEDERATED CAMPAIGNS*	0.003**	0.073***	−0.010**	0.052***	0.109***	
	0.037	0.000	0.043	0.000	0.000	

(continued on next page)

TABLE 5 (continued)

Dependent Variable	**ln *DIRECT DONATIONS*** (1)	**If *LARGE DONORS*** (2)	**ln *GOV GRANTS*** (3)	**If *GOV GRANTS* > 0** (4)	**ln *FEDERATED CAMPAIGNS*** (5)	**If *FEDERATED CAMPAIGNS* > 0** (6)
ln *DIRECT DONATIONS*	0.873***	0.418***	0.076***	−0.021	0.180***	0.094***
	0.000	0.000	0.000	0.139	0.000	0.000
Constant	0.809***	−4.271***	2.945***	−1.597***	6.669***	−3.017***
	0.000	0.000	0.000	0.006	0.000	0.000
Industry and Year	Yes	Yes	Yes	Yes	Yes	Yes
n	15,560	15,739	7,004	15,872	2,329	15,709
Adjusted or Pseudo R^2	0.8353	0.1855	0.4749	0.0549	0.2832	0.1861

*, **, *** Represent significance levels of 10 percent, 5 percent, and 1 percent, respectively (two-tailed).

The dependent variable measured at time *t* is: (1) ln *DIRECT DONATIONS*, (2) an indicator variable set to 1 if the organization has any large donors per the Form 990, Part IV (2), (3) ln *GOV GRANTS*, (4) an indicator variable set to 1 if *GOV GRANTS* is greater than 0, (4) ln *FEDERATED CAMPAIGNS*, and (5) an indicator variable set to 1 if *FEDERATED CAMPAIGNS* is greater than 0. The governance factors are detailed in Table 2. The other variables are defined in Table 3. The independent variables are measured at time *t*−1. Columns (1), (3), and (5) are estimated using OLS, while columns (2), (4), and (6) are estimated using probit. Coefficients and their respective p-values are presented. We use adjusted standard errors clustered by organizations and remove influential observations using studentized residuals greater than the absolute value of 3.

Column (3) of Table 5 reports that *Policies, Audit, Compensation*, *Management*, and *Access* are significantly associated with the level of government grants received, as expected. Surprisingly, *Board* is not. However, when we examine the probability of receiving any government grants, the coefficient on *Board* is significantly positive, as well as those on *Policies, Audit, Compensation, Management,* and *Access,* as reported in column (4). Overall, these results indicate that better governance is associated with more government funding.

Next, we examine the association between the governance factors and the level of federated fundraising receipts. Column (5) of Table 5 shows that the coefficients on *Compensation, Management,* and *Access* are significantly positive, consistent with predictions, but that the coefficients on *Policies, Audit, Board,* and *Minutes* are not, contrary to predictions. In the sixth column, we report results from estimating the probability of receiving any level of federated campaign funds as a function of our corporate governance factors. We report significantly positive coefficients on *Policies, Audit, Board, Access,* and *Minutes,* which indicate that organizations with these governance attributes are more likely to receive funding from federated campaigns. However, the coefficients on *Compensation* and *Management* are not significant. Untabulated results indicate that the mean governance factors are higher for organizations that receive federated campaign funds compared with the rest of the sample. Overall, our evidence suggests that federated campaigns provide assurance that some governance mechanisms are in place; however, there is less evidence that individual donors to these campaigns consider governance.

Governance and Donations Before and After the Revised Form 990

We next examine the link between total donations and governance in the periods before and after the 2008 Form 990 revision. The purpose of this analysis is two-fold. First, while our primary results show that the revised Form 990 governance disclosures are value-relevant, the analysis in this section investigates whether they have incremental information content. While donors had access to some governance information prior to 2008, the Form 990 revision made governance information publicly available to all potential donors and increased awareness of governance issues across the sector (Spector 2008).[17] Thus, if the Form 990 disclosures are used, then we expect governance to have an incrementally greater association with donations in the period after the Form 990 revision.

The second purpose of this analysis is to help rule out the possibility that our primary results are driven by an omitted correlated variable. The IRS revision to the Form 990 is an exogenous shock to the availability of governance information. The change in disclosure allows us to make a testable prediction that will hold if our primary results are due to donors using governance information, but, presumably, will not hold if our primary results stem from the influence of omitted variables. Specifically, we predict a stronger association between governance and donations after the 2008 change. If an omitted variable drives our result, then there will be no difference in the association between governance and donations before and after 2008.

In order to examine donor decisions prior to the Form 990 revision, we obtain financial data from 2005 to 2007 from the NCCS Core Financial file for the 10,846 organizations in our sample.

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[17] Most governance items appear on the Form 990 for the first time in 2008. However, the 2007 Form 990 did include questions regarding board size, related parties, changes to organizational documents, conflict of interest policy, and contact information. In addition, some donors obtained governance information privately from potential grantees and some organizations voluntarily disclosed certain governance items to the general public. Finally, some states required organizations to have certain governance policies in place. For example, beginning in 2005, California required large charities to have an independent audit committee. The fact that governance information is available to donors prior to 2008 does not affect our primary tests in Table 4, but may affect our ability to discern differences before and after the Form 990 revision.

We combine these data with the data from our main sample, which results in approximately 57,500 firm-year observations from 2005 through 2010 with all necessary data.[18] We refer to the years before the Form 990 revision (2005–2007) as the "pre-period" and the years following the Form 990 revision (2008–2010) as the "post-period." The first year of the post-period examines 2008 governance and 2009 donations.

We do not have access to actual governance disclosures prior to the Form 990 revision, so we assume that governance did not change over time, and use our 2008 governance factors to backfill the pre-period. To help ensure that our results are not driven by this assumption, we take a second approach, which is described later, that focuses on poor governance.

Governance interacted with post-990 revision. As seen in Table 6, we modify our model to compare the association between donations and governance before and after the Form 990 revision. *GOV FACTORS* for each of our seven factors is defined by an organization using actual governance in the post-period and 2008 governance in the pre-period. *POST* is an indicator variable set equal to 1 for observations in the post-period. We interact *POST* with each of our governance factors (*POST* $*$ *GOV FACTORS*) in seven respective models. We retain all of the other controls from our main model, including those for the other governance factors.

The coefficient on *GOV FACTORS* captures the effect of the given governance factor on total donations in the pre-period. To the extent that donors had access to governance information in the pre-period, we expect this coefficient to be positive. Our primary variable of interest is *POST* $*$ *GOV FACTORS*, which represents the incremental effect of governance on total donations after the Form 990 revision. To the extent that the governance information disclosed on Form 990 was incrementally useful to donors, we expect this coefficient to be positive.

Panel A of Table 6 reports a significantly positive coefficient on all of the *GOV FACTORS* except *Minutes*, indicating that the marginal donation reflects governance information in the pre-period. As predicted, the coefficient on the interaction term is positive and significant for *Policies, Audit, Compensation, Access,* and *Minutes,* consistent with the notion that information on these forms of governance had more influence on total donations once the revised Form 990 became available. The coefficient on *POST* is reliably negative, which is likely due to the fact that donations dropped significantly following the economic downturn across the entire sector.

Low governance interacted with post-990 revision. Our next approach challenges the assumption that governance did not change from the pre-period to the post-period. It is likely that some organizations did not have good governance in place in the pre-period, but implemented good governance just in time for the 2008 disclosure. However, it is less likely that organizations changed their governance for the worse immediately before the mandatory 2008 disclosure. In this subsection, we only assume that organizations that reported poor governance in 2008 had poor governance in the pre-period. For each factor, we define an indicator variable of *Q1 GOV FACTORS*, which we set equal to 1 if the organization reported governance in the lowest quartile for that factor in 2008, and 0 otherwise.

Our test is similar to the one reported in Table 6, Panel A except that, instead of using a continuous variable for each governance factor, we use the indicator variable *Q1 GOV FACTORS*. The coefficient on *Q1 GOV FACTORS* captures the effect of poor governance on donations in the pre-period. To the extent that donors had access to governance information in the pre-period, we expect this coefficient to be negative. Donors should be wary of supporting organizations that lack good governance. Our primary variable of interest, *POST* $*$ *Q1 GOV FACTORS*, represents the incremental effect of poor governance on donations after the Form 990 revision. To the extent that

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[18] The Core Financial file does not include program expense. Thus, for this test, we use the program ratio reported in 2008 for the pre-period. Results are robust to excluding the program ratio from the analysis.

TABLE 6

The Effect of Governance on Donations Before and After the Form 990 Revision

Panel A: Governance Interacted with Post-990 Revision

	Dependent Variable: ln *TOTAL DONATIONS*						
	Policies	*Audit*	*Compen-sation*	*Board*	*Manage-ment*	*Access*	*Minutes*
POST	−0.363***	−0.120***	−0.120***	−0.430***	−0.116***	−0.118***	−0.364***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
GOV FACTORS (defined in column heading)	0.110***	0.086***	0.138***	0.145***	0.078***	0.156***	0.007
	0.000	0.000	0.000	0.000	0.000	0.000	0.574
*POST * GOV FACTORS* (defined in column heading)	0.044***	0.025**	0.046***	−0.028	0.000	0.037***	0.036***
	0.000	0.014	0.000	0.413	0.968	0.000	0.000
Policies		0.122***	0.123***	0.124***	0.123***	0.123***	0.123***
		0.000	0.000	0.000	0.000	0.000	0.000
Audit	0.091***		0.092***	0.092***	0.092***	0.092***	0.092***
	0.000		0.000	0.000	0.000	0.000	0.000
Compensation	0.150***	0.150***		0.149***	0.150***	0.150***	0.150***
	0.000	0.000		0.000	0.000	0.000	0.000
Board	0.121***	0.121***	0.121***		0.121***	0.121***	0.121***
	0.000	0.000	0.000		0.000	0.000	0.000
Management	0.078***	0.078***	0.078***	0.077***		0.078***	0.078***
	0.000	0.000	0.000	0.000		0.000	0.000
Access	0.167***	0.166***	0.167***	0.163***	0.167***		0.166***
	0.000	0.000	0.000	0.000	0.000		0.000
Minutes	0.017	0.017	0.017	0.017	0.017	0.017	
	0.182	0.173	0.171	0.184	0.171	0.178	
PROGRAM RATIO	1.267***	1.265***	1.266***	1.269***	1.267***	1.266***	1.266***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
ln *FUNDRAISING EXP*	0.088***	0.088***	0.088***	0.088***	0.088***	0.088***	0.088***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
ln *TOTAL ASSETS*	0.333***	0.332***	0.333***	0.331***	0.333***	0.333***	0.333***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000

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TABLE 6 (continued)

	Dependent Variable: ln *TOTAL DONATIONS*						
	Policies	*Audit*	*Compensation*	*Board*	*Management*	*Access*	*Minutes*
ln *PROGRAM SERVICE REV*	−0.041***	−0.041***	−0.041***	−0.041***	−0.041***	−0.041***	−0.041***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
ln *AGE*	−0.080***	−0.080***	−0.080***	−0.082***	−0.080***	−0.080***	−0.080***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Constant	4.207***	4.208***	4.211***	4.305***	4.201***	4.196***	4.203***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Industry and Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	57,502	57,505	57,503	57,501	57,503	57,503	57,504
Adjusted R^2	0.2834	0.2832	0.2834	0.2835	0.2832	0.2834	0.2833

*, **, *** Represent significance levels of 10 percent, 5 percent, and 1 percent, respectively (two-tailed).

The governance factors are detailed in Table 2. The other variables are defined in Table 3, with the exception of *POST*, which is equal to 1 for the post-new Form 990 years 2009 and 2010. The dependent variable is measured at time *t*, and the independent variables are measured at time *t*−1. Coefficients and their respective p-values are presented. We use adjusted standard errors clustered by organizations and remove influential observations using studentized residuals greater than the absolute value of 3.

Panel B: Low Governance Interacted with Post-990 Revision

	Dependent Variable: ln *TOTAL DONATIONS*						
	Policies	*Audit*	*Compensation*	*Board*	*Management*	*Access*	*Minutes*
POST	−0.437***	−0.388***	−0.441***	−0.104***	−0.178***	−0.150***	−0.445***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Q1 GOV FACTORS (defined in column heading)	−0.200***	−0.189***	−0.253***	−0.166***	−0.018	−0.212***	0.005
	0.000	0.000	0.000	0.000	0.547	0.000	0.879
POST * *Q1 GOV FACTORS* (defined in column heading)	−0.087***	−0.043*	−0.064***	−0.065**	−0.005	−0.120***	−0.052**
	0.000	0.073	0.006	0.013	0.838	0.000	0.029
Policies Q1		−0.223***	−0.224***	−0.224***	−0.223***	−0.223***	−0.223***
		0.000	0.000	0.000	0.000	0.000	0.000
Audit Q1	−0.199***		−0.199***	−0.199***	−0.200***	−0.199***	−0.199***
	0.000		0.000	0.000	0.000	0.000	0.000

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TABLE 6 (continued)

Dependent Variable: ln *TOTAL DONATIONS*

	Policies	*Audit*	*Compen-sation*	*Board*	*Manage-ment*	*Access*	*Minutes*
Compensation Q1	−0.271***	−0.271***		−0.272***	−0.271***	−0.271***	−0.271***
	0.000	0.000		0.000	0.000	0.000	0.000
Board Q1	−0.185***	−0.185***	−0.185***		−0.185***	−0.184***	−0.185***
	0.000	0.000	0.000		0.000	0.000	0.000
Management Q1	−0.020	−0.020	−0.019	−0.020		−0.019	−0.019
	0.491	0.498	0.503	0.489		0.516	0.501
Access Q1	−0.245***	−0.245***	−0.245***	−0.244***	−0.245***		−0.245***
	0.000	0.000	0.000	0.000	0.000		0.000
Minutes Q1	−0.008	−0.008	−0.009	−0.009	−0.009	−0.008	
	0.797	0.789	0.779	0.782	0.782	0.791	
PROGRAM RATIO	1.320***	1.316***	1.316***	1.321***	1.316***	1.316***	1.317***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
ln *FUNDRAISING EXP*	0.094***	0.094***	0.094***	0.094***	0.094***	0.094***	0.094***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
ln *TOTAL ASSETS*	0.345***	0.345***	0.345***	0.345***	0.345***	0.345***	0.345***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
ln *PROGRAM SERVICE REV*	−0.041***	−0.041***	−0.041***	−0.041***	−0.041***	−0.041***	−0.041***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
ln *AGE*	−0.076***	−0.076***	−0.075***	−0.075***	−0.075***	−0.075***	−0.075***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Constant	4.295***	4.243***	4.302***	4.216***	4.302***	4.296***	4.302***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Industry and Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	57,509	57,508	57,509	57,508	57,507	57,507	57,509
Adjusted R^2	0.2689	0.2688	0.2687	0.2688	0.2687	0.2689	0.2687

*, **, *** Represent significance levels of 10 percent, 5 percent, and 1 percent, respectively (two-tailed).

The governance factors are detailed in Table 2. The other variables are defined in Table 3, with the exceptions of *POST*, which is equal to 1 for the post-new Form 990 years 2009 and 2010, and *Q1 GOV FACTORS*, which is equal to 1 if the organization falls in the first quartile of the governance factor in 2008. The dependent variable is measured at time *t*, and the independent variables are measured at time *t*−1. Coefficients and their respective p-values are presented. We use adjusted standard errors clustered by organizations and remove influential observations using studentized residuals greater than the absolute value of 3.

information on the Form 990 regarding low-quality governance is incrementally useful to donors, we expect this coefficient to be negative.

Results are reported in Table 6, Panel B. Not surprisingly, the coefficients on *Q1 GOV FACTORS* are reliably negative. Low governance is associated with fewer total donations in all periods. As predicted, the coefficient on the interaction term is negative and significant for *Policies, Audit, Compensation, Board, Access,* and *Minutes,* suggesting that poor governance had a more negative impact after the Form 990 revision.

Overall, we present evidence that governance information has a greater effect on donations following the Form 990 revision for five of our seven factors in Table 6, Panel A and six of our seven factors in Panel B. We do not find evidence that *Management* has an incremental effect in the post-period. Our results are generally consistent with the idea that information on the Form 990 is useful to donors and that our main results in Table 4 are not driven by omitted variables.

Board Size

In developing our factors, we assume more governance is better, which is reasonable for our structural measures that are measured as indicator variables. However, with respect to board size, which is measured as a continuous variable, we recognize that an optimal governance level may exist. With regard to size, although larger boards are more likely to possess the necessary expertise, very large boards can become unwieldy and disengaged and, therefore, less effective at monitoring management (Jensen 1993; Panel on the Nonprofit Sector 2007). The IRS (2008) notes that nonprofit boards should be neither too small nor too large, as small boards may not sufficiently represent broad public interest or have the resources to govern, and large boards may be inefficient. When we include *board_size* and $board\_size^2$ in our main model in an untabulated test, we find a significantly positive coefficient on *board_size* and significantly negative coefficient on $board\_size^2$. This result is consistent with the idea that donors recognize the costs and benefits to "more" governance and indicates that future research on optimal governance levels is warranted.

Another concern with board size is that boards may fulfill a governance role and a fundraising role, both of which can positively affect total donations. For example, a larger board may provide stronger oversight and may also have more wealthy individuals giving to and raising funds for the organization. Empirically, Aggarwal et al. (2012) report a positive association between donations and board size, which they attribute to donations from board members. This dual role may not be problematic for our tests because factor analysis extracts the common variance in the observable structural measures. Thus, our *Board* factor captures board size only to the extent that it has common variance with board independence.

Nevertheless, we address the concern that large boards are just better fundraisers by including *board_size* as an additional independent variable in our main model along with our *Board* factor, whereby the *Board* factor represents board governance and *board_size* represents board fundraising and governance. We find a significantly positive coefficient on *board_size* and also continue to find a significantly positive coefficient on the *Board* governance factor. These untabulated results indicate that the governance mechanisms reflected in our *Board* factor beyond board size are associated with donations, and suggest that the observed relation between *Board* and donations is not driven solely by larger boards raising more funds.

Changes in Governance Reported on the Form 990

In Table 7, we provide descriptive evidence on how, if at all, organizations modified their corporate governance policies once public disclosure of these policies became mandatory. This descriptive evidence is not the primary focus of our study, but it is relevant to public policy discussions regarding how best to regulate corporate governance. Ideally, we would like to examine

TABLE 7

Change in Observable Governance Structural Measures (n = 15,872)

Structural Measures	Change from 2008 to 2009 (n = 9,330 Organizations)			Change from 2009 to 2010 (n = 6,542 Organizations)		
	Decrease	No Change	Increase	Decrease	No Change	Increase
conflict	202	8,701	427	64	6,260	218
officers_conflicts	116	8,544	670	56	6,288	198
enforce_conflict	94	8,527	709	50	6,278	214
whistleblower	84	8,448	798	52	6,231	259
destruction	114	8,468	748	64	6,224	254
review_or_audit	195	8,071	1,064	129	6,266	147
audit_committee	369	7,687	1,274	181	6,096	265
ceo_salary	153	8,741	436	54	6,337	151
officer_salary	273	8,598	459	124	6,235	183
board_size	2,614	4,308	2,408	1,901	2,994	1,647
independence	1,099	6,963	1,268	595	5,231	716
no_relations	288	8,852	190	119	6,279	144
no_outsource	83	9,201	46	26	6,493	23
no_unreachable	47	9,151	132	31	6,468	43
own_web	135	8,964	231	61	6,324	157
contact_info	91	8,969	270	47	6,398	97
doc_comm	81	8,950	299	35	6,400	107
doc_gov	24	9,145	161	11	6,488	43
review_990	523	8,194	613	139	6,162	241
Total	6,585	158,482	12,203	3,739	115,452	5,107
% for year	3.7%	89.4%	6.9%	3.0%	92.9%	4.1%

All structural measures are defined in Table 1.

governance changes from 2007 to 2008, but 2007 governance measures are mostly unobservable. Nevertheless, examining the adoption of governance policies and procedures in the two years following the Form 990 revision provides some insight on how governance is evolving in the nonprofit sector.

Table 7 reports the number of observations in our sample that adopted a particular governance measure, repealed a particular governance measure, or made no change with respect to a particular governance measure.[19] For example, as seen in the first row, 427 organizations report having a conflict of interest policy in 2009, but not in 2008. On the other hand, only 202 organizations report eliminating such a policy from 2008 to 2009. Overall, there are few changes (i.e., in 2009, across all structural measures, 89.4 percent of the answers were identical to the prior year). Examining just the change observations, we do observe more organizations: (1) implementing conflict of interest, whistleblower, and document retention policies; (2) instituting audits and audit committees; (3) requiring salary approvals; (4) increasing board independence; (5) documenting meetings; (6) posting their Form 990s on their own website; and (7) having the board review their Form 990, than the reverse in 2009 and 2010. Organizations have apparently taken many of the suggestions in the report from the Panel on the Nonprofit Sector (2007) seriously. Interestingly, more boards decrease in size than increase in size.[20] Overall, the evidence suggests corporate governance in the nonprofit sector has generally increased since the 2008 Form 990 revision, but change is happening relatively slowly post-2008. It also indicates that uniform adoption of all governance polices by all organizations has not occurred.

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[19] We exclude *no_asset_diversion* and *no_doc_changes* from this analysis because these are one-time events rather than ongoing governance mechanisms.

Although we believe that our results convincingly show that donors value governance information, it is worthwhile to investigate a changes specification. That is, does a change in Form 990 disclosures cause a change in donations? We estimate a regression of the change in donations from 2009 to 2010 on the change in our seven governance factors from 2008 to 2009. We examine the change in governance factors both individually and simultaneously in an untabulated analysis and find *no* consistently significant coefficients with this specification. We offer two potential explanations for the lack of a statistically significant relation between the change in donations and the change in governance factors. As discussed above, there are relatively few governance changes during our sample period, and our tests may lack the statistical power to detect an association between changes in donations and governance. Alternatively, it is possible that donors are slow to obtain and process changes in governance information.

VI. CONCLUSIONS

We examine the role of corporate governance in donation decisions using the comprehensive list of governance questions from the revised Form 990. Our results indicate that six different governance mechanisms are associated with subsequent donations. Overall, our evidence suggests that the governance information is useful to individual donors, foundation grant makers, corporate giving officers, and government grantors.

To the best of our knowledge, this is the first study to investigate whether governance disclosures on the revised Form 990 are associated with giving decisions. We note two important caveats regarding our results. First, like most corporate governance studies, endogeneity is a concern. While our methodology addresses this concern in several ways, we cannot completely rule out the possibility that simultaneity or confounding variables influence our results. Second, our analysis is a first step and our results should not be interpreted as definitive evidence that all organizations should adopt all of the governance practices that we analyze. Decisions regarding governance require a careful cost-benefit analysis. In particular, our approach focuses on only one specific benefit, enhanced donations. Our evidence indicates that improved governance over executive compensation results in the largest increase in donations for our sample. Interestingly, a preliminary study by the IRS finds a similar benefit from governance concerning compensation with respect to tax compliance (Lerner 2012). In addition to attracting donors and improving tax compliance, other potential benefits of better governance may include more efficient operations and lower debt costs. Further research is needed to comprehensively capture the costs and benefits of nonprofit governance.

Additionally, other questions remain for future research. First, we consider only governance mechanisms listed on the Form 990. Other important governance mechanisms in the nonprofit sector include state charity bureaus and credit rating agencies, and these mechanisms may complement the governance factors we identify. Second, we generally assume more governance is better rather than attempting to identify an optimal level of governance. In reality, good governance is context-specific (Bhagat et al. 2008). As more data become available on the costs and benefits of particular policies in the nonprofit sector, more refined tests can investigate donor reactions to deviations from an optimal level of governance for particular types of organizations. Third, we do not assess whether the governance policies reported on the Form 990 are used in practice. Managers could simply be "window dressing" the Form 990 and not actually taking steps to ensure that they are fulfilling their fiduciary responsibilities. Finally, while we find that better governance is associated with more donations, we do not evaluate whether better governance improves an organization's actual performance. It is notoriously difficult, but vitally important, to measure how well an organization is serving its community and what factors, like corporate governance, can assist the organization in meeting its charitable mission.

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[20] For boards that decreased in size, the average board size before the change was 31.3 members. For boards that increased in size, the average board size before the change was 19.8 members. From Table 1, the average board size across our entire sample is 20.6. These changes are consistent with organizations moving toward a mean board size, which is perhaps optimal.

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Audit Fees and Social Capital

Anand Jha
Yu Chen
Texas A&M International University

ABSTRACT: We examine the impact of social capital on audit fees. We find that firms headquartered in U.S. counties with high social capital pay lower audit fees. Social capital measures the level of mutual trust in a region. Our results suggest that auditors judge the trustworthiness of their clients based on where the firm is headquartered and charge a premium when they trust the firm less. The basis of our results is the examination of more than 28,000 audit fees for more than 5,000 firms spanning the period of 2000 to 2009. The results are robust to controlling for a large number of firm-level and county-level characteristics.

Keywords: *audit fees; social capital; client risk; audit effort.*

JEL Classifications: *M42; M14.*

I. INTRODUCTION

In a seminal paper, Simunic (1980) considers a firm's audit fees to be dependent on the auditor's effort and the expected losses from litigation. Subsequently, researchers have investigated the impact of numerous variables as possible determinants of audit fees (Causholli, De Martinis, Hay, and Knechel 2010). These variables are expected to affect either the auditor's effort or litigation risk, both of which affect the audit fees. Prior research has not investigated the possible impact of the client firm's local social environment, which is the focal point of this study. Our purpose is to use a well-understood setting to investigate the role of social capital on economic decisions, a topic that is much less understood.

Social capital is often defined as the mutual trust in society. We propose that the social capital in the county where a U.S. firm is headquartered can have an impact on how much the auditors trust the managers of the firm. As we discuss below, auditors arguably have less trust when a firm is headquartered in a county with low social capital. We argue that this lack of trust will increase the auditor's effort and his or her fear of litigation and, therefore, will increase fees.

We test this idea by exploiting the variation in social capital at the county level in the United States. Using the zip code of the firm's headquarters, we gather data for variables proxying for the county-level

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social capital for each firm-year. We then conduct a regression analysis that examines the association between audit fees and the level of social capital where the firm is headquartered. In our analysis, we control for firm characteristics based on the audit-fee literature and also include a large set of controls at the county level. We find that audit fees are significantly lower in high social-capital counties. Our results are also economically significant because we find that a firm that is headquartered in a county with social capital in the 75th percentile pays about 12 percent less in audit fees compared to a firm headquartered in a county with social capital in the 25th percentile, *ceteris paribus*.

According to Simunic (1980), audit fees can increase due to more audit work and/or more expected losses. To further investigate which particular element drives up audit fees, we examine the impact of social capital on the auditor's report lag, which is a proxy for the auditor's effort, and on the firm's litigation risk, which is a proxy for the auditor's expected losses. We find that auditors take more time to sign off on their report for low social-capital clients. Furthermore, the probability of litigation involving the auditor is also higher in low social-capital counties.

We also conduct two tests of moderating effects that examine whether the influence of social capital is stronger under certain environments. First, we investigate whether the effect of social capital is stronger when the audit office is located closer to the client. The idea is that the auditors might have more confidence in their judgment of the client's trustworthiness when they reside closer. We find results consistent with our expectation because when the auditors are either located within a 100-kilometer (62.13 miles) radius of the client or in the same metropolitan statistical area (MSA) as the client, the effect of social capital on audit fees is tripled compared to when they are further away. Second, we investigate if the effect of social capital is stronger for the year 2004 and subsequent years, when auditing became more complex due to the Sarbanes-Oxley Act (SOX). We find that, indeed, social capital's effect is stronger post-2004.

Based on prior literature (Guiso, Sapienza, and Zingales 2004, 2008b; Grullon, Kanatas, and Weston 2010), we argue that these two additional results give us greater confidence that our results are causal instead of correlational. Taken together, these results provide strong evidence that auditors take into consideration the social capital of where the firms are headquartered in assessing their audit fees.

We note here that our results do not necessarily suggest that auditors are violating professional guidelines that require that they exercise "professional skepticism" in auditing their clients. Rather, the results suggest that the extent of the skepticism can vary based on where their clients are headquartered. Our results can be interpreted as indicating that auditors are prudent in their assessments because the social environment affects the quality of the financial reporting (Kang, Han, Salter, and Yoo 2010; McGuire, Omer, and Sharp 2012). Jha (2013), in particular, finds that when a firm is headquartered in a low social-capital county, the financial report's quality is poor. Specifically, the accrual management, real earnings management, propensity to commit financial fraud, and the "fogginess" of financial reports are all high. Prudence should dictate that auditors take into account the poor quality of reporting that can generally be expected of clients located in low social-capital counties, and that auditors be more skeptical in those cases. Our results are also consistent with the experimental and archival studies that show that auditors consider the integrity of the management when deciding how much effort to exert in auditing, and how much to charge their client (Beaulieu 2001).

By showing that the social capital where the firm is headquartered affects audit fees, our study makes an important contribution to the auditing literature. It shows that the social environment where the firm is headquartered can affect its relation with auditors and, consequently, the audit fees. This is a new way of looking at the auditor and client's relation. Although the audit-fee literature is extensive, no studies we know of have investigated the possible impact of the social environment on how much the auditors charge.

More broadly, our study contributes to an emerging strand of accounting literature that documents the effect of the social environment on managerial decisions (Hilary and Hui 2009; McGuire et al. 2012). We show that the social environment not only affects managerial decisions, but also the relations with other stakeholders, such as auditors. Further, this study is among the few in the accounting literature to examine the role of social capital on the firm's behavior. Although the concept of social capital is extensively studied in sociology, economics, management, and political science, the role of social capital in accounting settings is largely unexplored. Our study raises the possibility that other accounting decisions might be affected by the level of social capital where the firm is headquartered.

II. BACKGROUND AND HYPOTHESIS DEVELOPMENT

What is Social Capital?

Following Woolcock (2001), we define social capital as the norms and the networks that facilitate collective action. A high social-capital region has individuals with a greater propensity to honor an obligation and a greater mutual trust within a much denser network, all of which facilitate collective action.

The predominant approach in the economics literature is to view social capital as a "norm" that facilitates cooperation. Guiso et al. (2004) define social capital as the mutual level of trust and altruistic tendency in a society. Fukuyama (1997) defines social capital as "the existence of a certain set of informal values or norms shared among members of a group that permits cooperation among them." Portes (1998) defines social capital as the propensity to honor obligations. Guiso, Sapienza, and Zingales (2008a) provide a more comprehensive definition of social capital as "the set of beliefs and values that foster cooperation."

In contrast to the "norm" approach, many studies, particularly in the management literature, model social capital as a set of networks from which benefits are derived (Payne, Moore, Griffis, and Autry 2011). At first glance, these approaches appear as two distinct ways of viewing social capital. In fact, the distinction is not clear. The network definition also implicitly incorporates norms. A strong social network enhances the punishment for deviant behavior and encourages good behavior (Coleman 1994; Spagnolo 1999). A vigorous network fosters greater trust over time among its members and creates a culture that is more conducive to cooperation. Fukuyama (1997) notes that in a dense network, there are repeated games in which people rely on each other. Over time, this leads to a code of conduct in the society that encourages the propensity to honor obligations and develop mutual trust. Portes (1998) argues that, over time, these morals get passed from one generation to another and get internalized into society. Consequently, people feel obligated to behave in a certain way. Putnam (2001) provides a more detailed analysis of this view. Because of the difficulty of disentangling the effect due to norms versus a network, we do not make this distinction; instead, we focus on the common aspects of both the norms and the network views, following the Woolcock (2001) definition.

Social capital gained popularity after Coleman (1988) laid its theoretical foundation by drawing parallels with other types of capital, such as financial, physical, and human. Since then, a large body of the literature in different disciplines, such as economics, political science, and management, has examined the impact of social capital (Putnam 2000; Woolcock 2010; Payne et al. 2011). Documenting the dramatic rise in understanding the role of social capital, Woolcock (2010) notes that in the early 1980s, the phrase "social capital" was used in scholarly articles less than 100 times a year, and by 2008, it was used 16,000 times a year. This research shows that social capital is negatively associated with opportunistic behavior such as corruption (La Porta, Lopez-De-Silanes, Shleifer, and Vishny 1997), crime (Buonanno, Montolio, and Vanin 2009), and transaction costs associated with financial exchanges, such as buying stocks and getting loans (Guiso et al. 2004).

How Can Social Capital Affect Audit Fees?

Managers Are More Likely to be Honest in High Social-Capital Regions

The social norms of high social-capital regions induce managers to behave more honestly. A classic stream of literature supports the view that social norms affect individuals' decisions

(Milgram, Bickman, and Berkowitz 1969; Cialdini, Kallgren, and Reno 1991). This stream of literature argues that human beings develop a set of ideals for how they should behave based on what they see around them. When a person deviates from these ideals, there is a sense of guilt and, therefore, a cost. Managers might take this cost into account when making decisions (Akerlof 2007). Social norms are also self-enforcing (Hilary and Huang 2013) because there is a desire to conform to a group's expectations—partly by nature (Akerlof 2007; Hilary and Huang 2013) and partly because deviations are costly (Coleman 1994; Portes 1998).

The dense networks in high social-capital regions also encourage honest behavior from managers. In the context of managerial reporting behavior, a dense network means that stakeholders, such as institutional investors, bankers, and managers, are more likely to interact regularly with each other. More frequent interactions among these parties lead to greater information exchange and, therefore, more effective monitoring (Wu 2008), which subsequently leads to more truthful and forthcoming financial reporting.

Auditors Factor in the Manager's Honesty in the Fees They Charge

The two most important determinants of audit fees are the auditor's effort and the litigation risk (Simunic 1980; Venkataraman, Weber, and Willenborg 2008). Both are likely to be lower in a high social-capital environment, thus driving down the fees the auditors charge.

Social capital arguably affects auditor effort via the audit planning process. Examining entire populations of accounts is too costly, so auditors need to balance the costs and benefits of audit procedures. During the planning process, auditors identify more risky areas and spend more resources on those areas to reduce the audit risk. If auditors feel that management is more prone to misbehavior, then they conduct more substantive procedures to ensure that the financial statements are fairly presented (Beaulieu 2001). In contrast, if auditors trust their clients more, then they place greater reliance on internal controls and perform fewer substantive procedures to reduce their effort. When a firm is located in a high social-capital county, its managers are likely to be more forthcoming and honest in their financial reports, as discussed earlier and as documented in Jha (2013). If the firm's auditors have this perception, then they will trust the client to a greater extent and thereby reduce their total effort.[1]

Auditors also hire third parties to assess the integrity of management, review past financial information, and communicate with previous auditors (Rittenberg, Johnstone, and Gramling 2012). In a high social-capital county, because of its dense network and the honest reputation of its citizens, the auditor can obtain such evidence more easily, and probably with more precision. For example, the auditor might much more easily obtain a higher quality and quantity of audit evidence from banks, suppliers, customers, and other stakeholders in a high social-capital county. Therefore, the auditor is likely to expend less effort in obtaining sufficient and appropriate audit evidence, resulting in lower audit fees.

Auditors also perceive a higher lawsuit risk when firms are located in low social-capital counties because these firms are more likely to misbehave, and third parties might have less favorable opinions about the management. The fear of litigation risk can increase the audit fees because the cost of litigation relative to the audit fees can be severe. An example in Dye (1993) is illustrative: "Max Rothenberg & Company performed an audit review for $600 for 1136 Tenants' Corporation, but because of alleged deficiencies in the conduct of its engagement, was found liable for $232,278.30." A report from the Advisory Committee on the Auditing Profession (ACAP) notes that between 1996 and 2008, the six largest auditing firms paid approximately $5.16 billion to settle lawsuits (ACAP 2008). Given the concern about the risk and the cost associated with litigation relative to the fees charged, auditors are likely to take into consideration the trustworthiness of their client, even if the impact of the trust is only at the margin.

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[1] The idea that auditors adjust their extent of auditing according to how much they trust their clients is not new. Shaub (1996) notes that "trust is inherent in the audit process." He argues that auditors assess higher inherent risk and control risk when they trust their client less.

The discussion above suggests that in a low-trust environment, the auditor's effort and the litigation risk are both higher. Therefore, auditors likely will exert more effort and demand higher audit fees for firms located in low social-capital counties. Conversely, they will exert less effort and demand lower audit fees if a firm is headquartered in a high social-capital county.[2] Based on the above discussion, we propose the following hypothesis:

> **H:** *Ceteris paribus,* the firms headquartered in a high social-capital county pay lower audit fees.

This hypothesis rests on the idea that the firms headquartered in high social-capital counties have corporate cultures that are high in social capital. This concept is based on the recent accounting and finance literature on social environment and corporate decisions (Hilary and Hui 2009; Grullon et al. 2010). As in these studies, we borrow from the psychology literature and argue that firms hire and retain employees who share their own values and that employees prefer to work for firms that share their own values (Vroom 1966; Tom 1971; Holland 1976; McGuire et al. 2012). Over time, assuming that the key employees reside close to the firm's headquarters, these shared values mean that the culture of the headquarters also reflects the culture of its location. Therefore, if the county where the firm is headquartered has low social capital, then the auditor is likely to have less trust in managers employed at the firm's headquarters.[3]

III. RESEARCH METHOD AND DATA

Empirical Model

To test our hypothesis, we use a multivariate regression in which the dependent variable is the natural logarithm of the audit fees charged by the external auditor. The key variable of interest is the social capital of the county where the firm is headquartered:

$$\begin{aligned} LN(AUDIT\ FEE) = {} & \beta_0 + \beta_1 SOCIAL\ CAPITAL + \beta_2 LNASSETS + \beta_3 DEBT + \beta_4 ROA \\ & + \beta_5 BIG4 + \beta_6 LOSS + \beta_7 FISCAL\ YEAR\ END + \beta_8 DAYS\ TO\ SIGN \\ & + \beta_9 PUBLIC\ EXCHNG + \beta_{10} UNQUALIFED\ OPINION \\ & + \beta_{11} GOING\ CONCERN + \beta_{12} INHERENT\ RISK + \beta_{13} LITIGATION \\ & + \beta_{14} AUDITOR\ CHANGE + \beta_{15} SEGMENTS \\ & + \beta_{16} COUNTY\ PRESENCE + \beta_{17} LARGE\ SCALE + \beta_{18} SPECIALIST \\ & + \beta_{19} AUDITOR\ COMPETITION + \beta_{20} COST\ OF\ LIVING \\ & + \beta_{21} RELIGIOSITY + \beta_{22} DIST\ FROM\ SEC + \beta_{23} RURAL \\ & + \beta_{24} INCOME + \beta_{25} POPG + \beta_{26} LNPOP + \beta_{27} LITERACY \\ & + Industry\ Indicators + Year\ Indicators + \varepsilon. \end{aligned} \tag{1}$$

The variables are defined in detail in Appendix A. Our unit of analysis is a firm-year, where the subscript *it* is suppressed for expositional ease. The firm-level control variables are

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[2] We acknowledge that the county in which the auditor resides might also matter. We address this issue in additional analyses later in the paper.

[3] Firms can change their headquarters over time, and this change could add noise to our method. Although this noise is possible, firms seem to rarely change their headquarters. For example, Pirinsky and Wang (2006) find only 118 changes in headquarters in a sample of 5,000 firms spanning 15 years.

based on Hay, Knechel, and Wong (2006), who include controls for the size, complexity, inherent risk, profitability, leverage, auditor type, auditor report lag, and the busy season for audits. We expect audit fees to be higher for large, complex, and risky firms, when the firm is audited by the Big 4 audit firms and when the audit is conducted in a busy season. Because Fung, Gul, and Krishnan (2012) find that the city-level industry specialization of the auditors and their economies of scale have an impact on audit fees, we also add a measure for the city-level industry specialization and economies of scale. We also control for the extent of the audit market competition in the county, as in Newton, Wang, and Wilkins (2013), and the number of clients' geographic segments. Although the key managers who influence financial reporting are located in the firm's headquarters, the employees in other geographic locations also have an effect on the accounting information systems and the firm's financial reports. We control for this effect by including the number of geographic segments in our model. And, consistent with Fung et al. (2012), we add industry indicator variables based on the two-digit SIC code, as well as year indicator variables, to control for the impact from changes in the financial reporting regulations.

Because recent finance and accounting research shows that the religiosity in a county has a significant impact on the firm's financial reporting quality (Grullon et al. 2010; McGuire et al. 2012), we control for religiosity at the county level. We control for other related county-level characteristics, including the cost of living in the county, the distance from the nearest regional Securities and Exchange Commission (SEC) office, the population density, the county population, the population growth, the income per capita, and the literacy rate. We expect audit fees to be lower for firms that are located in religious counties because misconduct is expected to be lower in these counties. We also expect audit fees to be higher where the cost of living index is higher. The impact of the distance from the SEC is unclear. Firms further away from the SEC are likely to have more financial reporting irregularities, which results in higher audit fees. But firms further away from the SEC are also less prone to scrutiny by the SEC, reducing litigation risk and lowering audit fees.[4]

To control for the possibility that the error terms might be correlated, we cluster the standard errors at the county level. Because we cluster at the county level, we automatically control for clustering at the firm level (Bertrand, Duflo, and Mullainathan 2004; Dinc 2005; Cameron and Miller 2011).[5,6] In our main regression, we do not control for corporate governance characteristics, such as the characteristics of the board and the relative power of management compared to the shareholders, because doing so would severely limit our sample size. However, we do control for them in our robustness tests, and our results continue to hold. Because our entire sample is from the U.S., we automatically control for differences in legal origin, laws, and institutions.

Measure of Social Capital

We construct a social-capital index for each county following the steps in Rupasingha and Goetz (2008). They use two measures of norms and two measures of networks, and conduct a principal component analysis to construct an index for each county for the years 1997, 2005, and

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[4] Given that we have a large set of control variables, we check for multicollinearity by measuring the variance inflation factor (VIF). Because the VIFs of the explanatory variables are well below 10, and the average VIF of the explanatory variables is 2.42, multicollinearity does not appear to be a problem.

[5] In our case, the firms are nested in the counties. Therefore, we have a nested level of clustering. In such a case, "cluster-robust standard errors are computed at the most aggregate level of clustering" (Cameron and Miller 2011, 7).

[6] However, our main results continue to hold if we (1) cluster at the firm level, (2) adopt two-way clustering (firm and year or county and year) (Petersen 2009; Gow, Ormazabal, and Taylor 2010), or (3) use the Huber-White standard error without clustering.

2009. The social-capital index for each county and the underlying data used to construct the index are available at the Northeast Regional Center for Rural Development (NERCRD).[7]

As far as we know, the Rupasingha and Goetz (2008) approach to measuring social capital is the most comprehensive measure of social capital at the county level. Putnam (2007) uses their measure of social capital as an alternative measure for individual trust. Besides Putnam (2007), many authors in different disciplines have used Rupasingha and Goetz's (2008) index or followed their approach, including S. Deller and M. Deller (2010) and Hopkins (2011).

Following Rupasingha and Goetz (2008), the two measures of social norms we use are voter turnout in presidential elections and the census response rate. Higher values for these variables represent higher social capital. The literature has used both of these measures, either independently or as a component of a social-capital index. For example, Guiso et al. (2004) use participation in referenda in Italy as a measure of social capital, Alesina and La Ferrara (2000) use participation in a presidential election as a component in the construct of a social-capital index, and Knack (2002) uses the census response rate as a measure of social capital.

The two measures of networks are the number of social and civic associations and the number of nongovernment organizations (NGO) in counties. Social and civic associations include physical fitness facilities, public golf courses, religious organizations, sports clubs, managers and promoters, political organizations, professional organizations, business associations, and labor organizations in the county, but we exclude NGOs with an international focus. Both of these measures are normalized by the population in the county. The literature also uses these two measures independently as measures of social capital (Knack 2002; Hopkins 2011).

Because the measures of the norms and the network are highly correlated,[8] we follow Rupasingha and Goetz (2008) by conducting a principal component analysis to construct an index of social capital for each county for the years 1997, 2005, and 2009. We extract the first component as a measure of the social capital.[9] We then linearly interpolate the data to fill in the years 1998 to 2004 and 2006 to 2008,[10] following the same approach as Hilary and Hui (2009) and many other studies.[11] Appendix A provides a more detailed description of how we construct the social-capital measures.

We present the variation in social capital at the county level in Figure 1 for the year 2000. For brevity, we do not present the figures for 2005 and 2009 because they are similar. The correlation between the 2000 and the 2009 social-capital index is 0.91. This is consistent with the idea that unlike physical and human capital, social capital is "sticky" (Anheier and Gerhards 1995).

When constructing the social-capital index, we assume that all types of association memberships increase the general trust in the society. We acknowledge that not all researchers agree with this view. In the context of social capital, the common approach is to view associations as one of two types: those that act like "bridges" between groups, such as religious organizations, civic and social associations, bowling centers, physical fitness facilities, public golf courses, and sports clubs; and those that strengthen the "bonding" between members within a group, such as

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[7] The data is available upon request from Northeast Regional Center for Rural Development (NERCRD) at: http://aese.psu.edu/nercrd.

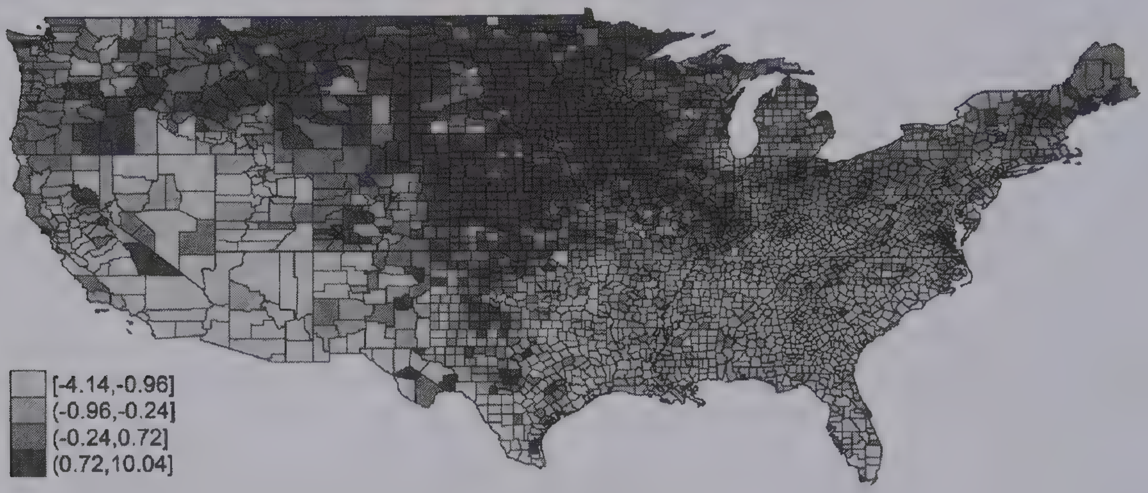
[8] For example, the correlation between the voter turnout in the election and number of organizations in the county is 0.30 in 2005.

[9] The eigenvalues of the first component for 1997, 2005, and 2009 are 2.06, 1.94, and 1.84, respectively. The eigenvalues of the other components are less than 1 except in 2009, when the second component has an eigenvalue of 1.03. To maintain consistency between the years, we use only the first component for each year and consider it the social-capital index.

[10] The results continue to hold when we use Rupasingha and Goetz's (2008) index and test for only those years for which their index is available.

[11] The use of linear interpolation to fill in the missing values of the in-between years is a common practice in the prior literature (Kumar, Page, and Spalt 2011; Alesina and La Ferrara 2000).

FIGURE 1
Social Capital at the County Level



This figure illustrates the variations in social capital as measured at the county level for 2000.

professional organizations, labor organizations, political organizations, and business associations, all of whose memberships are more exclusive.

This distinction is made because the bridging type is expected to generate mutual trust in the society, but whether the bonding type creates trust is unclear. Some researchers argue that a bonding-type association creates trust within the network, but might create distrust with those outside the network (Burt 1999, 2000). Some other researchers, such as Putnam (2007), disagree, arguing against treating bridging and bonding social capital as inversely related in a kind of zero-sum game. He goes on to show that people who have strong bonding social capital also appear to have more bridging social capital. For example, Putnam (2007) notes that the research shows that whites who have non-white friends also have more white friends. Brewer (1999) also finds that higher trust inside a group does not mean a lack of trust outside of the group.

We take the perspective of Putnam (2007) and Brewer (1999) in our study. We do not consider professional organizations, labor organizations, political organizations, and business associations as decreasing mutual trust in the society.

Sample and Descriptive Statistics

Our sample includes 28,634 firm-year observations that represent 5,167 firms and 57 industries, and spans the years 2000 to 2009. The sample is limited to these years because the audit-fee data in Audit Analytics are not available prior to 2000, and the social-capital data are not available beyond 2009.

We start with 31,595 observations representing the firm-years that have audit-fee data available in Audit Analytics and total assets data available in Compustat.[12] Further, we require that these firms be headquartered in the U.S. and belong to a nonfinancial and non-regulated industry.[13] From this set, we remove 66 observations because *SOCIAL CAPITAL* is not available, either because the firms are headquartered in Hawaii, the Virgin Islands, or Puerto Rico, or because we could not determine the county of the firm's headquarters based on the zip code. The unavailability of firm-level control variables further reduces the observations in the sample: 102 observations because *DEBT* is unavailable; 115 because *ROA* is unavailable; 248 because *FISCAL YEAR END* is unavailable; 374 because *INHERENT RISK* is unavailable; and 2,056 because *SEGMENTS* is unavailable. We winsorize all of the continuous variables in the model at the 1st and the 99th percentiles to mitigate the possible effects from outliers. We report the summary statistics of the sample in Panel A of Table 1.

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[12] We require that the audit fees be greater than zero and the total assets of firms in Compustat be greater than zero.

[13] Financial firms are those with an SIC classification between 6000 and 6999, and utility firms are those between 4000 and 4999.

IV. MAIN RESULTS

Univariate Results

The univariate results are consistent with our hypothesis that auditors charge firms with headquarters in high social-capital counties lower fees. We present this result graphically in Figure 2, in which we divide the sample into those firms located in high or low social-capital counties based on the median level of social capital. For both groups, we calculate the ratio of audit fees to total assets in dollars for each year and multiply it by 100. Figure 2 presents the mean values of this ratio. The figure shows that the audit fees per dollar of assets are higher for firms headquartered in low social-capital counties compared to those in high social-capital counties for every year. The mean audit fee per 100 dollars of assets for firms headquartered in low social-capital counties is 0.428. For high social-capital counties, it is 0.384, which is significantly lower ($p < 0.01$).

Next, we check the correlations between our variables of interest. The correlation of −0.03 between *SOCIAL CAPITAL* and *LN(AUDIT FEE)* is significant ($p < 0.01$), consistent with higher social capital being associated with lower audit fees. But *SOCIAL CAPITAL* is also significantly correlated with other county-level demographics, and these characteristics are significantly correlated with *LN(AUDIT FEE)*. For example, the correlations of *LN(AUDIT FEE)* with *COST OF LIVING, INCOME,* and *LITERACY* are 0.05, 0.21, and 0.08, respectively. They are all significant ($p < 0.01$). This result suggests a need to control for these characteristics in a regression framework to make a valid conclusion.

Multivariate Results

The result of the multivariate regression is also consistent with our hypothesis. The firms headquartered in a high social-capital county pay lower audit fees, *ceteris paribus*. Panel B of Table 1 presents the results from the multivariate regression analysis. The dependent variable is the natural logarithm of the audit fees. Columns (1) and (2), respectively, report the coefficients and the p-value of the main model summarized in Equation (1). The coefficient of −0.102 on *SOCIAL CAPITAL* is significant ($p < 0.01$). Based on this coefficient, we know that a firm headquartered in a county with social capital in the 75th percentile pays 88 percent of the audit fees paid by a firm headquartered in a county with social capital in the 25th percentile ($\exp(-0.102 * 0.101)/\exp(-0.102 * -1.173) = 0.88$), *ceteris paribus*.

To mitigate the concern that idiosyncrasies in certain time periods drive our results, we run the regression model for each year. We report the coefficient on *SOCIAL CAPITAL* from these regressions in Table 1, Panel C. *SOCIAL CAPITAL* is significant at the $p < 0.01$ level in each year, which suggests that our results are not inappropriately driven by certain years. Consistent with the audit-fee literature, audit fees are higher for larger firms and for firms in litigation-prone industries.[14]

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[14] The coefficient on *AUDITOR COMPETITION* is similar to Numan and Willekens (2012), who also use the Herfindahl index to measure competition. Unlike most studies, the coefficient of *BIG4* is not positive and significant. This is because we control for *COUNTY PRESENCE* (the natural logarithm of the sum of the audit fees that the auditor collected from the firms in the county), as in Fung et al. (2012), that is highly correlated with *BIG4* (0.43). The effect of *BIG4* is subsumed by *COUNTY PRESENCE*. In this respect, our results are consistent with Choi, Kim, Qiu, and Zang (2012). In untabulated results, we verify that when we remove *COUNTY PRESENCE* as an explanatory variable, *BIG4* becomes positively significant.

TABLE 1

The Impact of Social Capital on Audit Fees

Panel A: Summary Statistics (n = 28,634)

Variable	Mean	SD	1st Quartile	Median	3rd Quartile
LN(AUDIT FEE)	13.104	1.259	12.132	13.048	13.974
AUDIT FEE	1120000	1860000	186000	464000	1170000
SOCIAL CAPITAL	−0.496	0.905	−1.173	−0.472	0.101
CENSUS RESPONSE	0.706	0.055	0.67	0.716	0.746
VOTE	0.56	0.087	0.493	0.558	0.62
BRIDGING TYPE	0.446	0.399	0.243	0.339	0.506
BONDING TYPE	1.238	0.437	0.864	1.194	1.445
NGO	48.886	23.423	35.44	44.095	53.838
LNASSETS	5.552	1.948	4.204	5.522	6.857
ASSETS	2023.68	14840.8	66.955	250.044	950.513
DEBT	0.508	0.338	0.268	0.462	0.656
ROA	0.007	0.306	−0.012	0.094	0.155
BIG4	0.82	0.384	1	1	1
LOSS	0.265	0.441	0	0	1
FISCAL YEAR END	0.666	0.472	0	1	1
DAYS TO SIGN	62.752	26.868	47	61	74
PUBLIC EXCHNG	0.84	0.366	1	1	1
UNQUALIFIED OPINION	0.536	0.499	0	1	1
GOING CONCERN	0.053	0.224	0	0	0
INHERENT RISK	0.264	0.193	0.107	0.235	0.382
LITIGATION	0.435	0.496	0	0	1
AUDITOR CHANGE	0.079	0.269	0	0	0
SEGMENTS	2.32	0.833	1.732	1.732	3
COUNTY PRESENCE	15.542	1.915	14.156	15.745	17.009
LARGE SCALE	0.318	0.388	0.01	0.01	0.64
SPECIALIST	0.586	0.492	0	1	1
AUDITOR COMPETITION	0.388	0.197	0.266	0.311	0.431
COST OF LIVING	125.614	39.813	104.9	115.5	134.4
RELIGIOSITY	0.521	0.11	0.434	0.525	0.59
DIST FROM SEC	2.666	2.559	0.485	2.055	3.579
RURAL	0.501	0.5	0	1	1
INCOME	44.856	13.88	35.6	42.252	50.42
LNPOP	13.71	1.121	13.206	13.75	14.33
POPG	0.833	1.19	0.083	0.645	1.406
LITERACY	34.662	9.378	27.267	33.733	42.011

Panel B: Main Regression Analysis

	Dependent Variable = *LN(AUDIT FEE)*	
	(1) Coeff.	(2) p-value
SOCIAL CAPITAL	−0.102	(<0.001)
LNASSETS	0.463	(<0.001)
DEBT	0.224	(<0.001)

(continued on next page)

TABLE 1 (continued)

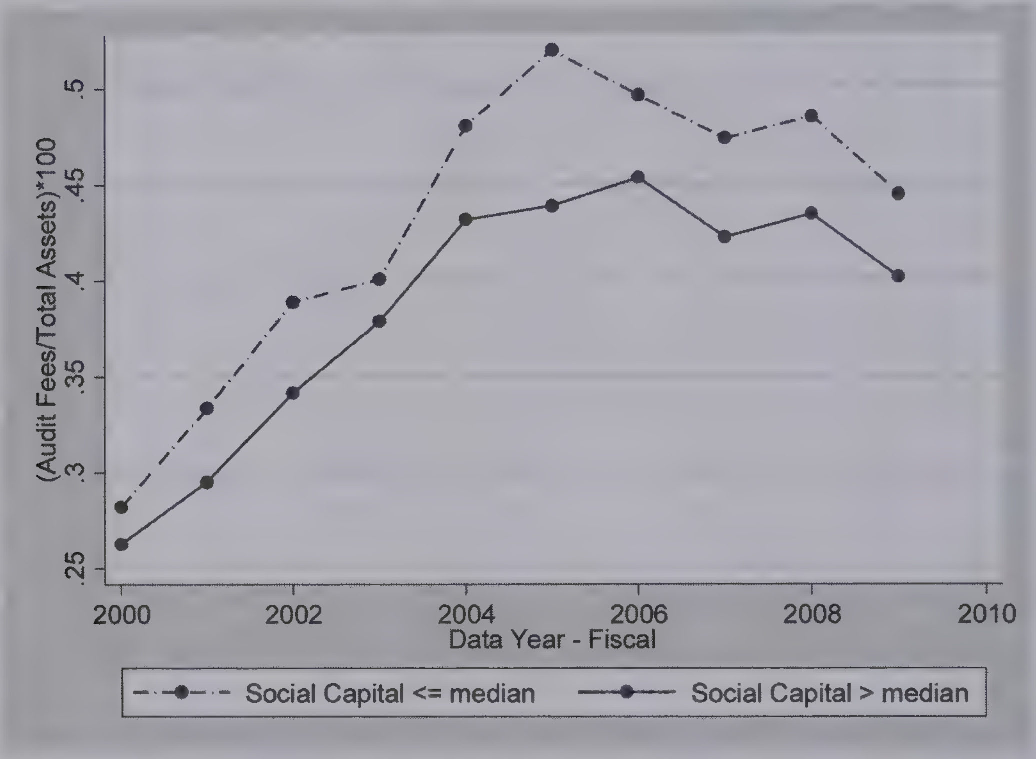
	Dependent Variable = *LN(AUDIT FEE)*	
	(1) Coeff.	(2) p-value
ROA	−0.304	(<0.001)
BIG4	0.011	(0.553)
LOSS	0.022	(0.231)
FISCAL YEAR END	0.124	(<0.001)
DAYS TO SIGN	0.003	(<0.001)
PUBLIC EXCHNG	0.071	(<0.001)
UNQUALIFIED OPINION	−0.095	(<0.001)
GOING CONCERN	0.051	(0.044)
INHERENT RISK	0.560	(<0.001)
LITIGATION	0.098	(<0.001)
AUDITOR CHANGE	−0.094	(<0.001)
SEGMENTS	0.117	(<0.001)
COUNTY PRESENCE	0.120	(<0.001)
LARGE SCALE	−0.135	(<0.001)
SPECIALIST	0.152	(<0.001)
AUDITOR COMPETITION	−0.103	(0.079)
COST OF LIVING	0.001	(0.252)
RELIGIOSITY	−0.199	(0.032)
DIST FROM SEC	−0.016	(<0.001)
RURAL	0.034	(0.146)
INCOME	−0.001	(0.726)
LNPOP	−0.078	(<0.001)
POPG	−0.017	(0.015)
LITERACY	0.004	(0.019)
Industry Indicators	Yes	
Year Indicators	Yes	
Observations	28,634	
R^2	0.810	

Panel C: Regression Analysis by Year

Year	Coeff. of *SOCIAL CAPITAL*	p-value
2000	−0.090	(<0.001)
2001	−0.085	(<0.001)
2002	−0.094	(<0.001)
2003	−0.095	(<0.001)
2004	−0.104	(<0.001)
2005	−0.114	(<0.001)
2006	−0.144	(<0.001)
2007	−0.118	(<0.001)
2008	−0.154	(<0.001)
2009	−0.103	(<0.001)

Panel A reports the summary statistics. Panel B reports the results of the main regression of the model. Panel C reports the results from rerunning the main regression for each year separately. For brevity, Panel C only reports the coefficients on *SOCIAL CAPITAL*. The p-values are based on standard errors clustered at the county level.
A more detailed description of the variables is in Appendix A.

FIGURE 2
Audit Fees and Social Capital



This figure presents the mean percentage of audit fees charged per dollar of assets of a firm headquartered in a low social-capital county (social capital $\leq$ median) versus a high social-capital county (social capital $>$ median).

V. FURTHER ANALYSES

The Channels by Which Social Capital Affects Audit Fees

In this section, we attempt to provide further insight into why audit fees are lower in a high social-capital environment. Specifically, we examine whether the auditor's effort and the perceived litigation risk are lower when the social capital is high. Indeed, we find that the auditor's effort and the probability of lawsuits are both lower when firms are headquartered in a high social-capital county.

Lower Auditor's Effort in Counties with High Social Capital

To examine the impact of social capital on the auditor's effort, we use the lag between the auditor's signature date and the date of the fiscal year-end (*DAYS TO SIGN*) as a measure of the auditor's effort. A better measure of the auditor's effort is the number of hours put into the audit work. However, these data are not publicly available. We follow the audit literature (Knechel and Payne 2001; Ettredge, Li, and Sun 2006) and use *DAYS TO SIGN* as a proxy for the auditor's effort.[15]

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[15] Knechel and Payne (2001) use a proprietary database and find that the incremental audit effort is positively associated with the audit report lag.

TABLE 2

The Lower Auditor's Effort in High Social-Capital Counties

	Dependent Variable = *DAYS TO SIGN*	
	(1) Coeff.	(2) p-value
SOCIAL CAPITAL	−1.901	(<0.001)
LNASSETS	−3.357	(<0.001)
DEBT	9.355	(<0.001)
ROA	1.328	(0.320)
BIG4	0.284	(0.695)
LOSS	2.201	(0.005)
FISCAL YEAR END	0.802	(0.074)
PUBLIC EXCHNG	−6.174	(<0.001)
UNQUALIFIED OPINION	−3.661	(<0.001)
GOING CONCERN	6.013	(<0.001)
INHERENT RISK	1.955	(0.319)
LITIGATION	−1.761	(0.030)
AUDITOR CHANGE	6.791	(<0.001)
SEGMENTS	0.447	(0.128)
COUNTY PRESENCE	0.951	(0.001)
LARGE SCALE	−1.146	(0.162)
SPECIALIST	1.144	(0.017)
AUDITOR COMPETITION	−3.286	(0.072)
COST OF LIVING	0.041	(<0.001)
RELIGIOSITY	6.558	(0.013)
DIST FROM SEC	−0.201	(0.049)
RURAL	−0.413	(0.560)
INCOME	−0.090	(0.038)
LNPOP	−1.378	(0.006)
POPG	0.531	(0.069)
LITERACY	0.015	(0.746)
Industry Indicators	Yes	
Year Indicators	Yes	
Observations	28,634	
R^2	0.218	

The table reports the results of a regression analysis that examines the association between the auditor's effort and social capital. The p-values are based on the standard errors clustered at the county level.
A more detailed description of the variables is in Appendix A.

To test whether *DAYS TO SIGN* is larger for firms in low social-capital counties, we conduct a regression analysis. Table 2 presents the results. The dependent variable in these regressions is *DAYS TO SIGN* and, following Chan, K. Chen, T.-Y. Chen, and Yu (2012), the control variables are the same as those used to examine *LN*(*AUDIT FEE*). The coefficient on *SOCIAL CAPITAL* is −1.901 in Column (1) and is significant ($p < 0.01$). This finding suggests that a one-standard-deviation change in *SOCIAL CAPITAL* is associated with about 1.7 fewer days ($-1.901 * 0.905 = -1.720$) spent on auditing.

Lower Litigation Risk in Counties with High Social Capital

To examine the impact of social capital on litigation, we obtain Audit Analytics data on the litigations filed against the firms, as well as the auditors. We construct an indicator variable, *LAWSUIT*, that equals 1 for firm-years with litigation against the firm, and 0 otherwise.[16] We then conduct a logit test in which the dependent variable is *LAWSUIT*, and the variable of interest is *SOCIAL CAPITAL*. After controlling for various county-level characteristics and firm-level variables that might affect the propensity for litigation, we examine whether a negative association exists between social capital and litigation.

As expected, we find that a negative association does exist. Column (1) in Table 3 reports the pooled logit analysis without industry fixed effects. We do so to maintain the same number of observations as other pooled tests. Using industry fixed effects results in a loss of observations for industries that have no lawsuits filed. The coefficient on *SOCIAL CAPITAL* is −0.215 and is significant ($p < 0.01$). The odds ratio based on this coefficient is 0.81 (exp (−0.215)), suggesting that a one-unit change in social capital is associated with about a 19 percent lower probability of litigation. The impact of a one-standard-deviation change in social capital is similar because the standard deviation for *SOCIAL CAPITAL* is close to 1. The results are similar when we add industry fixed effects and Tobin's Q as a control variable (Table 3, Column (3)), or when we add the same set of control variables as in Tables 1 and 2 (Table 3, Column (5)).

Moderating Tests

We conduct two moderating tests to investigate whether the effect of social capital on audit fees is stronger when the distance between the auditor and the firm is greater, and when the complexity of the auditing process is greater. Following prior literature (Guiso et al. 2004, 2008b; Grullon et al. 2010), we argue that if the social capital has a stronger effect on the audit fees in certain environments compared to others, and if the direction of the effect is as expected given theory, then this provides further assurance that our explanation for the association between the social capital and the audit fees is correct.

The Effect of Social Capital is Stronger when Auditors Reside Close to the Firm

If social capital affects audit fees, then it is likely to have a stronger effect when the auditors are located closer to the firm. Choi et al. (2012) show that a shorter distance between the client and the auditor reduces the information asymmetry and improves the audit quality. Arguably, one reason a shorter distance improves audit quality is because the collection and processing of information is easier. Auditors are more likely to be aware of the "soft information," such as information bearing upon the trustworthiness and reputation of the firm's managers, and to have greater confidence in managers' judgments when the auditors reside closer to the firm (Liberti 2005; Mian 2006). Furthermore, local auditors can make use of the dense networks in high social-capital counties and more easily collect other relevant soft information about the firm from other stakeholders, such as bankers, suppliers, and customers. Therefore, the effect of the client's social capital on assessing audit fees is likely to be stronger.

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[16] We code *LAWSUIT* as 1 if, in any given year, a lawsuit is initiated that is coded in one of the following Audit Analytics categories: Accounting and Auditing Enforcement Release (category 54), Accounting Malpractice (category 2), or Financial Reporting (Category 48), and 0 otherwise. Our *LAWSUIT* variable captures lawsuits filed against clients, not their auditors. We focus on the likelihood of the client being sued because it is a comprehensive measure of the client risk that auditors face. It captures both the financial reporting risk and the client business risk—both of which increase audit fees.

TABLE 3

The Lower Litigation Risk in High Social-Capital Counties

	Dependent Variable = *LAWSUIT*					
	(1) Coeff.	**(2) p-value**	**(3) Coeff.**	**(4) p-value**	**(5) Coeff.**	**(6) p-value**
SOCIAL CAPITAL	−0.215	(−0.005)	−0.235	(−0.007)	−0.174	(−0.029)
LNASSETS	0.404	(<0.001)	0.434	(<0.001)	0.489	(<0.001)
DEBT	−0.004	(0.979)	0.161	(0.354)	−0.247	(0.104)
ROA	−0.829	(<0.001)	−1.009	(<0.001)	−0.853	(<0.001)
BIG4	−0.319	(0.041)	−0.291	(0.067)	−0.306	(0.051)
LOSS	0.633	(<0.001)	0.644	(<0.001)	0.664	(<0.001)
FISCAL YEAR END					0.01	(0.924)
DAYS TO SIGN					0.012	(<0.001)
PUBLIC EXCHNG					0.186	(0.135)
UNQUALIFIED OPINION					−0.265	(0.018)
GOING CONCERN					0.292	(0.129)
INHERENT RISK	0.645	(0.013)	0.745	(0.027)	0.791	(0.009)
LITIGATION	0.522	(<0.001)	0.575	(<0.001)	0.637	(<0.001)
AUDITOR CHANGE					0.269	(0.061)
SEGMENTS					0.008	(0.881)
COUNTY PRESENCE	0.107	(0.012)	0.115	(0.008)	0.102	(0.015)
LARGE SCALE	0.059	(0.655)	−0.014	(0.919)	−0.052	(0.696)
SPECIALIST	0.211	(0.032)	0.248	(0.021)	0.218	(0.031)
AUDITOR COMPETITION	−0.309	(0.401)	−0.342	(0.365)	−0.143	(0.690)
COST OF LIVING	0.002	(0.392)	0.003	(0.170)	0.002	(0.457)
RELIGIOSITY	−0.112	(0.827)	−0.072	(0.884)	−0.208	(0.669)
DIST FROM SEC	−0.012	(0.561)	0.004	(0.835)	−0.006	(0.750)
RURAL	0.189	(0.097)	0.187	(0.094)	0.147	(0.160)
INCOME	0	(0.980)	−0.002	(0.859)	0.002	(0.868)
LNPOP	−0.058	(0.491)	−0.071	(0.424)	−0.034	(0.680)
POPG	−0.019	(0.650)	−0.018	(0.685)	−0.008	(0.863)
LITERACY	0.004	(0.721)	−0.004	(0.706)	−0.004	(0.720)
Q			−0.096	(0.008)		
Industry Indicators			Yes		Yes	
Year Indicators	Yes		Yes		Yes	
Pseudo R^2	0.071		0.097		0.116	
	28,634		25,717		28,251	

The table reports the coefficients from the logit tests. The p-values are based on the standard errors clustered at the county level. A more detailed description of the variables is in Appendix A.

We find the effect of social capital is stronger when auditors are located within 100 kilometers of the firm or in the same MSA. We obtain the auditor's county based on the city of the auditor's office provided in Audit Analytics.[17] We collect the data on MSAs from the Census Bureau.[18] We then calculate the spatial distance between the county where the firm has its headquarters and the county where the firm's auditor is located. Based on Choi et al. (2012), we divide the firms into those within a 100-kilometer radius or in the same MSA, and those that are neither within a 100-kilometer radius nor in the same MSA.[19,20]

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[17] The sample for this test is reduced by 313 observations because either the auditor's city is unavailable in the Audit Analytics database, or it is not possible to determine the auditor's county based on the information provided.

[18] See: https://www.census.gov

We rerun our main tests for these two groups separately and test whether the coefficient on *SOCIAL CAPITAL* is larger when auditors are located closer to the firm. We find that the coefficient is −0.116 when the auditor is near versus only −0.038 when the auditor resides further away (Table 4, Panel A, Columns (1) and (3), respectively). This difference is statistically significant—the p-value for the F-test is 0.013.

The Effect of Social Capital is Stronger When Auditing is Complex

In a complex auditing environment, auditors face a much more difficult task in auditing financial reports. Under such an environment, the incremental value of soft information such as social capital could be greater. In other words, social capital will have a stronger effect when auditing is complex.

The Sarbanes-Oxley Act of 2002 (SOX) allows us to test this possibility because after SOX, auditing became more complex. Managers of some firms responded to SOX by shifting to earnings management techniques that are more difficult to detect (Cohen, Dey, and Lys 2008). More importantly, the new rules increased the responsibilities of the auditor. In this respect, the key rule is Section 404, which requires auditors to audit the effectiveness of the client's internal control as of year-end. Passed in 2002, this section was implemented after 2004.[21] A number of studies document a significant rise in audit fees in 2004 and beyond (Raghunandan and Rama 2006; Ghosh and Pawlewicz 2009).

Using years 2004 and beyond as a regime when the auditing environment was more complex, we find that social capital indeed appears to have a stronger effect for audits done in 2004 and later. To conduct this test, we divide the sample into audits done during the less complex environment before 2004 versus audits done in the more complex environment in 2004 and later. In order to have similar sample sizes between the two groups, we limit the post-2004 sample to 2008. We rerun our main tests separately for the two groups to test whether the coefficient of *SOCIAL CAPITAL* is greater when auditing is more complex. The coefficient is −0.122 when auditing is complex (Table 4, Panel B, Column (3)), versus −0.097, when auditing is not as complex (Table 4, Panel B, Column (1)). The difference is marginally significant ($p < 0.10$) based on an F-test.

VI. ROBUSTNESS CHECK

The Results are Robust when We Use Region Fixed Effect

Figure 1 shows that high social-capital counties are concentrated mainly in the Northeast and Midwestern states. One concern is that there might be some regional variables that we are missing in our model and that these omitted variables might bias our results.

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[19] The results are robust when we use 75 kilometers instead of 100 kilometers as the cutoff when determining whether the auditors are close to the client.

[20] Choi et al. (2012) point out that considering firms in the same MSA as being close, but not taking into account the distance can be problematic because such a classification might falsely classify the auditor in an adjacent MSA that might be close to the client as being distant.

[21] Accelerated filers (usually large firms) began to comply only after November 15, 2004. The non-accelerated firms (usually small, foreign firms) started to comply even later by starting in 2006 and ending in 2010. In their Table 1, Gao, Wu, and Zimmerman (2009) provide much detail about the chronology of the compliance schedule for Section 404 of SOX.

TABLE 4

Moderating Tests

Panel A: The Stronger Effect of Social Capital When Auditor Resides Close to the Firm

	Dependent Variable = *LN*(*AUDIT FEE*)			
	Auditors Close (<100 km or same MSA)		**Auditors Far (≥ 100 km and not same MSA)**	
	(1) Coeff.	**(2) p-value**	**(3) Coeff.**	**(4) p-value**
SOCIAL CAPITAL	−0.116	(<0.001)	−0.038	(0.090)
Control variables	Yes		Yes	
Industry Indicators	Yes		Yes	
Year Indicators	Yes		Yes	
F-test (p-value)				
Coeff. of *SOCIAL CAPITAL* when auditor is close = Coeff. of *SOCIAL CAPITAL* when auditor is far	(0.013)			
Observations	22,177		6,144	
R^2	0.825		0.819	

Panel B: The Stronger Effect of Social Capital When Auditing is Complex

	Dependent Variable= *LN*(*AUDIT FEE*)			
	Less Complex Auditing		**More Complex Auditing**	
	(1) Coeff.	**(2) p-value**	**(3) Coeff.**	**(4) p-value**
SOCIAL CAPITAL	−0.097	(<0.001)	−0.122	(<0.001)
Control variables	Yes		Yes	
Industry Indicators	Yes		Yes	
Year Indicators	Yes		Yes	
F-test (p-value)				
Coeff. of *SOCIAL CAPITAL* when auditing is less complex = Coeff. of *SOCIAL CAPITAL* when auditing is complex	(0.100)			
Observations	12,589		13,807	
R^2	0.763		0.779	

The table reports the coefficients from the OLS test. The control variables are the same as those in Panel B of Table 1. The p-values are based on the standard errors clustered at the county level. Less complex auditing refers to 2000 to 2003, and more complex auditing refers to 2004 to 2008.

A more detailed description of the variables is in Appendix A.

To mitigate this concern, we construct indicator variables that capture whether the firm is headquartered in the West (AZ, AK, CA, CO, HI, ID, MT, NM, NV, OR, UT, WA, WY), the Northeast (CT, MA, ME, NH, NJ, NY, PA, RI, VT), the South (AL, AR, DC, DE, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA, WV), or the Midwest (IA, IL, IN, KS, MI, MN, MO, ND, NE, OH, SD, WI), based on the U.S. census classifications. We add these indicator variables to our main model. This, in essence, controls for *all* of the time-invariant characteristics that might be specific to a certain region. We continue to find that firms headquartered in high social-capital counties have lower audit fees (Table 5, Panel A, Column (1))—the coefficient for *SOCIAL CAPITAL* is negative and significant at the 1 percent level. This addition of regional indicators absorbs some of the effect of the social capital because the social capital is similar within a particular region and does not change much over time. But despite this partial absorption of the social-capital effect, we find that local social capital affects audit fees.

Another way to ensure that our results are not driven by differences across regions is to conduct a separate analysis for each region and verify that *SOCIAL CAPITAL* continues to have an effect for each region. When we run a separate regression for each region, the coefficient for *SOCIAL CAPITAL* continues to be negative and significant for all regions (Table 5, Panel A, Columns (3) to (10)). Furthermore, the size of the coefficient also appears to be similar; except for the Southern and Western states, there is no statistical difference in the size of the coefficients of *SOCIAL CAPITAL* (Table 5, Panel B). This gives us more confidence that we are not missing variables that are region-specific in our model.

The Results are Robust when We Use Rupasingha and Goetz's (2008) Social-Capital Index

Recall that we use the updated version of the underlying data that Rupasingha and Goetz (2008) compiled and made available on the NERCRD website. Furthermore, we follow their steps to construct our own index for social capital. Since their data are available only for 1997, 2005, and 2009, we construct our index for these particular years and use linear interpolation to fill in the intervening years.

To ensure that our results are not driven by the way we construct the measure of social capital or by linear interpolation, we rerun the main test using the Rupasingha and Goetz (2008) social-capital index. To be conservative, when running these tests, we limit the firms to only those firms that have data available for 2005, as well as 2009.[22] Despite a drop of over 90 percent in the sample size, the results continue to hold for both those years. They are reported in Table 6. For brevity, we do not tabulate the control variables. They are similar to those in the main results reported in Panel B of Table 1.

The Results are Robust When We Add Corporate Governance Controls

Better governance leads to low business risk and better internal controls and, in turn, to lower audit fees. To ensure that our results are not driven by corporate governance measures rather than social capital, we add more control variables and rerun the main regression. These control variables consist of the following: *DUALITY*, an indicator variable equal to 1 if the CEO is also the chair of the board; *INDP DIRECTOR*, the percentage of independent directors on the board; and *G1*, *G2*, and *G3*, which are indicator variables based on the governance index constructed by Gompers, Ishii, and Metrick (2003). The Gompers index (G-index) measures how much power managers have compared to shareholders. A lower value of the index represents more power for the shareholders.

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[22] We do so because there are fewer firms in Compustat and Audit Analytics in 2009 than in 2005. This is because after SOX, due to greater compliance costs, many publicly traded firms started "going dark" (that is, firms stopped reporting to the SEC) or turned private (Engel, Hayes, and Wang 2007).

TABLE 5

The Effects of *SOCIAL CAPITAL* are Robust When We Use Regional Fixed Effects and for Subsamples Based on Region

Panel A: Results from Multiple Regressions

Dependent Variable= *LN(AUDIT FEE)*

	Pooled		Midwest		Northeast		West		South	
	(1) Coeff.	(2) p-value	(3) Coeff.	(4) p-value	(5) Coeff.	(6) p-value	(7) Coeff.	(8) p-value	(9) Coeff.	(10) p-value
SOCIAL CAPITAL	−0.078	(<0.001)	−0.092	(0.006)	−0.101	(0.015)	−0.132	(<0.001)	−0.039	(0.094)
Control Variables	Yes		Yes		Yes		Yes		Yes	
Industry Indicators	Yes		Yes		Yes		Yes		Yes	
Year Indicators	Yes		Yes		Yes		Yes		Yes	
Regional Fixed Effects	Yes									
Observations	28,634		5,440		7,309		7,907		7,978	
R^2	0.821		0.858		0.847		0.813		0.814	

Panel B: F-tests Based on Subsamples by Regions

F-tests	p-value
Coeff. of *SOCIAL CAPITAL* in Midwest = Coeff. of *SOCIAL CAPITAL* in Northeast	(0.859)
Coeff. of *SOCIAL CAPITAL* in Midwest = Coeff. of *SOCIAL CAPITAL* in West	(0.418)
Coeff. of *SOCIAL CAPITAL* in Midwest = Coeff. of *SOCIAL CAPITAL* in South	(0.190)
Coeff. of *SOCIAL CAPITAL* in Northeast = Coeff. of *SOCIAL CAPITAL* in South	(0.183)
Coeff. of *SOCIAL CAPITAL* in West = Coeff. of *SOCIAL CAPITAL* in South	(0.036)

Panel A of the table reports the coefficients from the main regression with regional dummies and for each region separately. Control variables are the same as those in Panel B of Table 1. The p-values are based on the standard errors clustered at the county level. Panel B reports the p-values of the F-tests.
A more detailed description of the variables is in Appendix A.

TABLE 6

Results are Robust When We Use Rupasingha and Goetz's (2008) Index of Social Capital

	Dependent Variable= *LN(AUDIT FEE)*			
	Year = 2005		Year = 2009	
	(1) Coeff.	(2) p-value	(3) Coeff.	(4) p-value
SOCIAL CAPITAL (Rupasingha and Goetz 2008)	−0.092	(<0.001)	−0.111	(<0.001)
Control variables	Yes		Yes	
Industry Indicators	Yes		Yes	
Year Indicators	Yes		Yes	
Observations	1,840		1,840	
R^2	0.790		0.832	

The table reports the coefficients from regression analyses when the social-capital index constructed by Rupasingha and Goetz (2008) is used. The sample is limited to only those firms that are common to both 2005 and 2009. The control variables are the same as in Panel B of Table 1. The p-values are based on the standard errors clustered at the county level.

A more detailed description of the variables is in Appendix A.

As in Bergstresser and Philippon (2006), when the G-index is not available, we replace it with that of the previous year. Following their study, we construct four indicator variables based on this score. The addition of these variables reduces the sample size greatly, but the results continue to hold. In Table 7, we report the results of this robustness test. The coefficient for *SOCIAL CAPITAL* continues to be negative and significant (p-value < 0.01), and the magnitude of the coefficient, although slightly lower, is similar to those reported in the main results.

The Results are Robust When We Use a Dichotomous Measure of Social Capital

In order to mitigate concerns that our results could be driven by a measurement error in *SOCIAL CAPITAL*, we construct an indicator variable that is equal to 1 if *SOCIAL CAPITAL* is greater than the median, and 0 otherwise. We rerun all of the tests using this indicator variable for *SOCIAL CAPITAL* instead of the continuous measure. All of the results are qualitatively similar. For brevity, we do not report these results.

VII. DISCUSSIONS

The Impact of Religiosity Compared to Social Capital

Given the recent literature that documents how a religious culture induces ethical and less risky behavior, one natural question that arises is why we focus exclusively on the impact of social capital and not on the impact of religiosity.

We do so because the impact of religiosity on audit fees is not as clear. For example, both accrual management (Gul, Chen, and Tsui 2003) and real earnings management (Sohn 2011) are expected to increase audit fees, but religiosity decreases accrual management and increases real earnings management (McGuire et al. 2012). So, if earnings management moderates audit fees with respect to religiosity, then which direction the relation takes is not clear. In contrast,

TABLE 7
Results are Robust When We Control for the Corporate Governance Variables

	Dependent Variable= *LN(AUDIT FEE)*	
	(1) Coeff.	(2) p-value
SOCIAL CAPITAL	−0.080	(0.002)
Control variables	Yes	Yes
Additional controls:		
Q	−0.019	(0.033)
DUALITY	−0.018	(0.423)
INDP DIRECTOR	0.311	(0.001)
G1	−0.155	(0.003)
G2	−0.145	(<0.001)
G3	−0.116	(0.001)
Industry Indicators	Yes	
Year Indicators	Yes	
Regional Indicators	Yes	
Observations	4,756	
R^2	0.822	

The table reports the coefficients from the main regression using more controls in addition to the control variables in Panel B of Table 1. The p-values are based on the standard errors clustered at the county level.
A more detailed description of the variables is in Appendix A.

high social capital is associated with low accrual management, as well as real earnings management (Jha 2013), which suggests a definitive direction in which social capital might affect audit fees.

However, the more important reason we focus on social capital is that it is directly related to trust, more so than religiosity, and, as we have discussed in this study, trust is likely to affect the auditor's effort, as well as the perceived litigation risk.

Nevertheless, because religiosity is associated with financial reporting irregularities, in all of our tests, we incorporate religiosity as a possible explanatory variable. We find the coefficient for *RELIGIOSITY* to be negative and significant in our main results, which suggests that the audit fee is lower in more religious counties.

In line with our expectation, we find that the economic impact of *RELIGIOSITY* is about three times lower compared to *SOCIAL CAPITAL*. In our main results (Column (1), Panel B of Table 1), the coefficient for *RELIGIOSITY* is −0.199. In terms of economic magnitude, this coefficient means that firms headquartered in a county with religiosity in the 75th percentile are charged 0.97 times the audit fees of those firms in the 25th percentile ($\exp(-0.199 * 0.59)/\exp(-0.199 * 0.434) = 0.97$). Furthermore, the coefficient for *SOCIAL CAPITAL* shows that a similar change in social capital results in 0.88 times lower audit fees when headquartered in the 75th percentile compared to those headquartered in the 25th percentile.

Arguably, we find a small impact for *RELIGIOSITY* because our measure of *SOCIAL CAPITAL* subsumes some of the effect of *RELIGIOSITY*; by construction, *SOCIAL CAPITAL* incorporates the number of churches in the county. To address this concern, we exclude *SOCIAL CAPITAL* and rerun the main model. The magnitude of *RELIGIOSITY* increases, but only slightly to −0.257 (untabulated results). The economic impact of *RELIGIOSITY* still appears to be considerably less compared to the impact of *SOCIAL CAPITAL*. This impact shows that the smaller coefficient for *RELIGIOSITY* is not because *SOCIAL CAPITAL* subsumes the impact of religiosity, but because the impact of *SOCIAL CAPITAL* is much stronger compared to that of *RELIGIOSITY*.

What is the Effect of the Auditor's Social Capital?

We focus on the effect of the firm's local social capital on audit fees. But, arguably, the auditor's local social capital might also have an effect. One could argue that how much the auditor trusts the client might originate from the auditor rather than from the firm (Shaub 1996). For example, auditors from low social-capital counties might tend to have less trust in their clients regardless of where the clients are located. Another possibility is that the lack of trust originates from both sides. When the client and the auditor both have low social capital, the relation between the auditor and the client might be even more trust-deficient. In other words, the combined social capital of the auditor and the firm might have a stronger effect on audit fees.

In untabulated tests, we find that in addition to the firm's social capital, the social capital where the auditor is located also has an effect. Furthermore, the average of the social capital of the firm and the auditor has a slightly stronger effect. However, we view this result with caution because, for about 77 percent of the firms, the auditors are within 100 kilometers of their clients. These are often the same or adjacent counties with similar levels of social capital. This similarity makes it empirically difficult, if not impossible, to investigate which of the two effects is more important and to what extent they attenuate each other. We leave it to future research to examine these questions.

Is Social Capital Negatively Associated with Crime?

The key argument in our study is that a firm's managers from a high social-capital county have a higher propensity to honor their obligations and, therefore, auditors view them as being more trustworthy. This propensity suggests that one should expect lower crime rates in high social-capital counties. As mentioned earlier, prior studies do find that the crime rate is negatively associated with social capital (Buonanno et al. 2009; Rosenfeld, Baumer, and Messner 2001). Jha (2013), in particular, finds that the propensity to commit financial fraud is lower when a firm is headquartered in a high social-capital county. We also check whether there is an association between other sorts of crime in the county and the social capital in the dataset we use in this study and, indeed, we find this to be the case. The correlation between property crime per capita and social capital is –0.184, and the correlation between violent crime and social capital is –0.261. Both are significant ($p < 0.01$).

VIII. CONCLUSION

The two main determinants of audit fees are the auditor's effort and the litigation risk associated with auditing. Both of these determinants are likely to be higher when there is less mutual trust between the firm's managers and the external auditor. Therefore, lack of trust can increase the fees that the auditors charge. In this study, we investigate whether a social environment that indirectly measures mutual trust can affect audit fees.

Our research question is motivated by two independent streams of the literature. First, an emerging strand of the literature shows that the social environment where the firm is headquartered permeates the culture of the firm and affects its managers' behavior. Second, the social-capital literature shows that mutual trust and the propensity to cooperate can differ across regions and that such differences can affect the transaction costs of financial exchanges.

Building on these two streams of literature, we argue that auditors trust the managers of firms headquartered in a low social-capital county less and suggest that auditors charge these firms a fee premium. Our testable hypothesis is that audit fees are higher for firms in a low social-capital county.

We test this hypothesis by constructing a social-capital index for each county-year and examining whether a firm headquartered in a low social-capital county pays higher audit fees, holding other variables constant. Our results are consistent with our hypothesis, indicating that a firm located in a low social-capital county pays higher audit fees. We find that the result is robust to a wide range of controls and is economically nontrivial. A deeper analysis examining the mechanism by which social capital affects audit fees shows that in low social-capital counties, both the auditor's effort and the litigation risk are higher.

The contribution of this study is two-fold. First, it shows that the social environment can affect the firm-auditor relation, not just corporate decision making. Second, the study suggests that trust is an important component of the auditor-client relation, that local social capital proxies to some extent for auditor trust, and its impact on audit fees is meaningful. We leave it to future research to examine the impact of other nonfinancial forces that can affect audit fees.

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APPENDIX A

Variables	**Description**
Main Dependent Variable	
LN(AUDIT FEE)	This variable is the natural logarithm of the total audit fee of the primary external auditor. *Source: Audit Analytics*
Dependent Variable for Supporting Tests	
LAWSUIT	An indicator variable that is equal to 1 if there is a lawsuit against the firm in that year. We code *LAWSUIT* as 1 if, in any given year, the firm is classified in the following categories: Accounting and Auditing Enforcement Release (category 54), Accounting Malpractice (category 2), Financial Reporting (category 48), and 0 otherwise. *Source: Audit Analytics*
DAYS TO SIGN	This variable is the lag between the signature date of the audit opinion and the date of the fiscal year-end (SIG_DATE_OF_OP_S − FISCAL_YEAR_END_OP). *Source: Audit Analytics*

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APPENDIX A (continued)

Variables	Description
Main Research Variable	
SOCIAL CAPITAL	This variable is the measure of the social capital at the county level. It is constructed following Rupasingha and Goetz (2008). Specifically, the variable is constructed by using the first component from a principal component analysis that uses four different measures. For example, we use the following four measures: assn97, nccs97, pvote96, and respn00 for 1997, where assn97 is the sum of the religious organizations, civic and social associations, business associations, political organizations, professional organizations, labor organizations, bowling centers, physical fitness facilities, public golf courses, sport clubs, managers and promoters, membership sports and recreation clubs, and membership organizations not elsewhere classified in 1997. We divide the total by 12 because there are 12 different categories. Further, we also divide it by the population of the county. We then multiply it by 10,000. The nccs97 is the total number of nongovernment organizations, excluding the ones with an international focus, in 1997 divided by the population multiplied by 10,000. The pvote96 is the number of votes cast divided by the population above 18 times 100. The respn00 is the census response rate. Then we use a principal component analysis and consider the first component to construct one index for each county. A similar approach is used for 2005 and 2009. For each of these years, the presidential elections and census response closest to 2005 and 2009 are used, respectively. We then linearly interpolate and fill the social capital data for the in-between years. *Source: Northeast Regional Center for Rural Development (NERCRD), Rupasingha and Goetz (2008)*
Firm-Level Controls used in Model (1)	
LNASSETS	This variable is the natural logarithm of total assets (ln(AT)). *Source: Compustat*
DEBT	This variable is the ratio of total liabilities to total assets (LT/AT). *Source: Compustat*
ROA	This variable is the ratio of net income to total assets (EBITDA/AT). *Source: Compustat*
BIG4	This indicator variable equals 1 if the primary auditor is one of the following: PricewaterhouseCoopers, Ernst & Young, Deloitte & Touche, or KPMG, and 0 otherwise. *Source: Compustat and Audit Analytics*
LOSS	An indicator variable that is equal to 1 if the *ROA* is negative, and 0 otherwise. *Source: Compustat*
FISCAL YEAR END	This is an indicator variable that is equal to 1 if the fiscal year ends in December, and 0 otherwise. *Source: Audit Analytics*
DAYS TO SIGN	This variable is the lag between the signature date of the audit opinion and the date of fiscal year-end (SIG_DATE_OF_OP_S – FISCAL_YEAR_END_OP). *Source: Audit Analytics*
PUBLIC EXCHNG	This indicator variable equals 1 if the firm is a public company trading in a major exchange (Compustat data item STKO = 0), and 0 otherwise. Firms that are coded as 0 are either subsidiaries of the public firms or are not traded on one of the major exchanges. *Source: Compustat*

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APPENDIX A (continued)

Variables	Description
UNQUALIFIED OPINION	This indicator variable equals 1 if the auditor issues an unqualified opinion without any additional language (Compustat data item AUOP = 1), and 0 otherwise. We code the firms that have an unqualified audit opinion with additional language as 0. This approach is in line with Landsman, Nelson, and Rountree (2009). *Source: Compustat*
GOING CONCERN	This indicator variable equals 1 if the auditor issues a going concern modified report, and 0 otherwise. *Source: Audit Analytics*
INHERENT RISK	This is the sum of receivables and inventory and scaled by assets ((RECT + INVT)/AT). *Source: Compustat*
LITIGATION	This indicator variable equals 1 if the SIC code of the firm is one of the following: 833–2836, 3570–3577, 3600–3674, 5200–5961, 7370–7374, or 8731–8734. *Source: Compustat*
AUDITOR CHANGE	This indicator variable equals 1 if the auditor had changed in the fiscal year, and 0 otherwise. *Source: Compustat*
SEGMENTS	This is the square root of the number of geographic segments. *Source: Compustat Legacy Segment*
COUNTY PRESENCE	This variable measures the presence of the auditor in the county and is constructed as in Fung et al. (2012). It is the natural logarithm of the sum of the audit fees that the auditor collected from the firms in the county. *Source: Compustat and Audit Analytics*
LARGE SCALE	This variable measures the scale of the auditor as in Fung et al. (2012). To construct this measure, we first construct *NCLIENT*, which is the number of clients the auditor has in each two-digit SIC industry by county-year. We then rank *NCLIENT* in percentile for each year and divide it by 100. Therefore, this measure captures how large-scale the auditor is from an industry and location perspective. *Source: Compustat and Audit Analytics*
SPECIALIST	This variable measures whether the auditor is a specialist and is constructed as in Fung et al. (2012). It is an indicator variable that is equal to 1 if the ratio of the total fees collected by the auditor for the industry to the total fees collected is the highest, and 0 otherwise. *Source: Compustat and Audit Analytics*
AUDITOR COMPETITION	This variable measures the auditor's competition in the county. It is calculated as in Newton et al. (2013). Specifically, it is calculated as follows: $\Sigma_{i=1}^{N}\left[\frac{s_i}{S}\right]$, where N is the number of audit firms in a county; S is the sum of the audit fees collected by the auditors in the county; and s_i is the audit fees collected by auditor i in the county. *Source: Compustat and Audit Analytics*
Firm-Level Controls used in Robustness Tests	
Q	This variable is the Tobin's Q. It is calculated as follows: (PRCC_C * CSHO + AT − CEQ − TXDB)/AT, where PRCC_C is the closing price of the stock in the calendar year; CSHO is the common shares outstanding; CEQ is the common equity; and TXDB is the deferred tax. *Source: Compustat*
DUALITY	This indicator variable equals 1 if the CEO is also the chairman of the board of directors. *Source: ExecuComp*

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APPENDIX A (continued)

Variables	Description
INDP DIRECTOR	The percentage of independent directors on the board. *Source: Investor Responsibility Research Center (IRRC)*
G1–G4	We construct four indicator variables based on the governance index constructed by Gompers et al. (2003). Their index was constructed in the 1990s for S&P 1500 firms by using 24 different governance characteristics. A smaller value means that the shareholders have greater rights. The *G1* equals 1 if the governance index is less than or equal to 6, and 0 otherwise. The *G2* equals 1 if the index is more than 6 and less than or equal to 9, and 0 otherwise. The *G3* equals 1 if the index is more than 9 and less than or equal to 12, and 0 otherwise. The *G4* equals 1 if the index is more than 13, and 0 otherwise. As in Bergstresser and Philippon (2006), when the G-index is not available, it is replaced with the previous year's. *Source: Investor Responsibility Research Center (IRRC)*
County-Level Controls	
COST OF LIVING	This variable measures the cost of living index of a county for 2013. *Source: http://www.coli.org/*
RELIGIOSITY	This variable is the percentage of religious adherents in the county, measured as in Hilary and Hui (2009). *Source: Association of Religion Data Archive (ARDA)*
DIST FROM SEC	This variable is the distance to the closest SEC regional office. As in Kedia and Rajgopal (2011), the SEC headquarters considered is in Washington, DC, and the regional offices are located in New York City, NY; Miami, FL; Chicago, IL; Denver, CO; and Los Angeles, CA. As in their study, we use a formula that uses the longitude and latitude of the two points to calculate the geographic distance. *Source: U.S. Department of Commerce , Bureau of Economic Analysis (BEA)*
RURAL	This variable is an indicator variable that is equal to 1 if the county's population density is less than the median and 0 otherwise. The population density is the ratio of the population to the land area. *Source: BEA*
INCOME	This variable is the income per capita in a county deflated by the Consumer Price Index. *Source: Census Bureau*
LNPOP	This variable is the natural log of the county's population. *Source: BEA*
POPG	This variable is the percentage of the population growth of the county since the previous year. *Source: BEA*
LITERACY	This variable is the percentage of persons 25 years and over with a bachelor's degree or higher in the U.S. county population. *Source: Census Bureau*

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Rating Agency Adjustments to GAAP Financial Statements and Their Effect on Ratings and Credit Spreads

Pepa Kraft
New York University

ABSTRACT: I examine a dataset of both quantitative (hard) adjustments to firms' reported U.S. GAAP financial statement numbers and qualitative (soft) adjustments to firms' credit ratings that Moody's develops and uses in its credit rating process. I first document differences between firms' reported and Moody's adjusted numbers that are both large and frequent across firms. For example, primarily because of upward adjustments to interest expense and debt attributable to firms' off-balance sheet debt, on average, adjusted coverage (cash flow-to-debt) ratios are 27 percent (8 percent) lower and adjusted leverage ratios are 70 percent higher than the corresponding U.S. GAAP ratios. I then find that Moody's hard and soft rating adjustments are associated with significantly higher credit spreads and flatter credit spread term structures. Overall, the results indicate that Moody's quantitative adjustments to financial statement numbers and qualitative adjustments to credit ratings enable it to better capture default risk, consistent with it effectively processing both hard and soft information.

Keywords: *rating agency; off-balance sheet finance; corporate credit risk; soft information.*

I. INTRODUCTION

This paper examines the rating process for corporate issuer ratings. Ratings are used for valuation purposes, in contracts, and in regulation (Beaver, Shakespeare, and Soliman 2006). Credit rating agencies have provided ratings for over a century. Their economic

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function is to reduce duplication of information-processing costs by debt investors (Wakeman 1984).[1] Accounting numbers are a major input into issuer ratings. However, rating agencies do not use the accounting numbers reported by firms in their raw form. Instead, rating agency manuals state that rating agencies make "analytical adjustments to better portray reality" and "to better reflect the underlying economics of transactions and events" (Standard & Poor's 2008, 22; Moody's 2006, 3). This study provides empirical evidence on the scope and magnitude of these adjustments and tests whether rating agency adjustments capture credit risk.

I classify rating agency adjustments into hard and soft adjustments, following Petersen (2004). Information is defined as hard if it can be reduced to numbers and is easy to transmit. Moody's quantitative adjustments to reported financial numbers fall into this category because they capture its credit analysts' assessments of credit risk stemming from quantitative risk factors, such as the magnitude of off-balance sheet debt (Moody's 2006). In contrast, information is defined as soft if it is qualitative by nature. Moody's assessments of credit risk stemming from qualitative risk factors fall into this category.

Moody's categorizes corporate issuers by industry. For each industry, Moody's defines a matrix of quantitative factors, such as leverage, profitability, and size. Credit analysts evaluate each borrower on all those dimensions. The matrix generates two hypothetical ratings. The first hypothetical rating is the matrix output using financial ratios that are calculated on the basis of U.S. GAAP (generally accepted accounting principles) reported numbers. The second hypothetical rating is the output using financial ratios that include quantitative adjustments by the credit analysts, "hard adjustments." Hard adjustments primarily include adjustments of reported numbers for operating leases, hybrid securities, securitizations, defined benefit pensions, capitalized interest, and unusual and nonrecurring items (Moody's 2006). The adjusted accounting numbers serve as input to Moody's ratio analysis, which generates the hypothetical rating based on adjusted leverage, profitability, and other ratios.

In addition to hard adjustments, rating agencies assess qualitative factors of credit risk (Standard & Poor's 2008). Soft assessments are meant to incorporate factors such as management quality, aggressive accounting, weak controls, governance risk, industry structure, and managerial bondholder friendliness (Moody's 2007). The rating agency assigns a numerical score to this information and thereby "hardens" it. I infer the size of the soft adjustment from the difference between the hypothetical rating based on adjusted financial ratios and the actual rating that Moody's awards to the issuer, where only the latter is public information.

I propose two hypotheses about how hard and soft rating agency adjustments are associated with credit spreads and credit spread term structures. First, I hypothesize that greater rating agency adjustments are priced in credit spreads. This hypothesis may appear simplistic given the debate in the accounting literature as to whether disclosures relating to non-recognized items such as footnotes are priced by equity and debt investors. However, rating agency adjustments involve various degrees of discretion in that they represent the rating agency's interpretation of borrowers' disclosures. Second, I hypothesize that greater rating agency adjustments are associated with a flatter credit term structure. Duffie and Lando (2001) show theoretically that debt investors' uncertainty about the value of a firm's assets and, thus, about how close the firm is to default, raises short-term credit spreads relative to longer-term spreads, flattening the credit spread term structure. Yu (2005) and Arora, Richardson, and Tuna (2011) provide empirical support for this theoretical result as they find that greater uncertainty is associated with flatter credit spread term structures. I predict that greater rating agency adjustments imply greater uncertainty about the borrower and, thus, make it harder for debt investors to assess the value of the firm's assets and, thus, how close the firm is to default. Hence, I predict that greater rating agency adjustments are associated with flatter credit term structures.

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[1] Investors rely on the rating agencies' reputations to produce accurate ratings (Klein and Leffler 1981; Shapiro 1983; White 2002; Strausz 2005.)

Using Moody's Financial Metrics database for the sample years 2002 to 2008, I find the most important adjustment to reported U.S. GAAP numbers is the recognition of off-balance sheet debt (*OFFBSD*).[2] Specifically, rating agency adjustments capture substantial amounts of off-balance sheet debt for nonfinancial firms. The average leverage (coverage) ratio increases (decreases) by 70 percent (27 percent), and for 98 percent of the sample observations, the dollar amount of recognized debt increases. Across all industries, more than 91 percent of all firms have operating leases that Moody's treats as debt and, thus, capitalizes. The average soft adjustment decreases the rating by 0.38 notches, where one notch is the difference between two consecutive ratings; for example, the difference between A+ and A. The combined effect of the hard and soft adjustments, which becomes the total adjustment, lowers the rating by 0.83 notches.

I first provide evidence on the construct validity of rating agency adjustments. Predictably, I find that rating agency adjustments to recognized GAAP numbers and qualitative assessments of credit risk are associated with lower ratings. Rating agency adjustments significantly improve the explanatory power of credit risk models for ratings.

Consistent with the first hypothesis, I find that quantitative, soft, and total rating agency adjustments are significantly positively associated with credit spreads. A one-standard-deviation increase in each of these three adjustments is associated with an increase of 16 percent, 39 percent, and 35 percent increases, respectively, in credit spreads. A non-nested model test reveals that models based on adjusted accounting numbers better explain credit spreads than models based on reported numbers. This evidence implies that ratings are more than mechanical mappings of firm characteristics; instead, they also incorporate the rating agency's assessments of credit risk arising from hard and soft factors. The rating agency's total adjustments and its qualitative assessment of credit risk arising from soft factors evidently capture true default risk, given that they are priced in the public debt market.

Consistent with the second hypothesis, I find that quantitative, soft, and total rating agency adjustments are significantly positively associated with a flatter credit spread term structure, the ratio of short-term to long-term credit spreads. A one-standard-deviation increase in soft (total) rating agency adjustments is associated with an increase of 4.7 percent (3.5 percent) in the average firm's ratio of the five- to 20-year credit spreads of 71.0 percent. This evidence supports the theory in Duffie and Lando (2001), which suggests that uncertainty with respect to asset measurement can give rise to greater short-term credit spreads relative to long-term credit spreads, resulting in a flatter credit spread term structure.

The study contributes to the literature in two ways, primarily concerning the debate about the role of rating agencies. The paper describes the rating process for corporate issuers and provides empirical evidence for the magnitude and scope of adjustments for hard and soft factors. The evidence is consistent with the view that rating agencies are effective processors of accounting information in the realm of traditional credit risk assessments of corporate issuers. Consistent with Petersen's (2004) conjecture, I show that credit ratings are a mapping of both hard and soft information; that is, while a large part of the rating is a function of reported numbers, qualitative factors enter, as well, and are associated with the market's assessment of default risk. My findings also imply that ratings can serve as a comprehensive contracting device to incorporate off-balance sheet debt adjustments into rating-based covenants, such as rating-trigger clauses or performance-based pricing, as an alternative to contracting directly on accounting ratios.[3]

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[2] Moody's Financial Metrics provides Moody's adjustments to financial statement items as the net line-by-line differences in reported and adjusted balance sheets, income statements, and cash flow statements for corporate borrowers.

Second, the paper contributes to the literature on off-balance sheet financing by providing new evidence on the widespread extent and magnitude of disclosed off-balance sheet debt by utilizing the Financial Metrics dataset.[4] The new proposed accounting rules on lease accounting, as discussed in the joint project between the Financial Accounting Standards Board (FASB) and the International Accounting Standards Board (IASB), are likely to substantially increase leverage ratios, unless firms restructure their financings (Imhoff and Thomas 1988).

Overall, the empirical results are consistent with rating agency adjustments to recognized U.S. GAAP numbers and rating agency assessments of qualitative credit risk improving the explanatory power of credit risk models. The results are robust to a variety of controls for firm characteristics. However, two caveats are important in interpreting the empirical results. First, the association between credit spreads and rating agency adjustments could be mechanical, if credit spreads are a simple function of ratings. This concern is mitigated, however, because credit spreads likely incorporate timelier information than ratings (Hull, Predescu, and White 2004; Norden and Weber 2004) and incorporate more comprehensive information sets (Collin-Dufresne, Goldstein, and Martin 2001). Second, it is also possible that rating analysts use credit spreads in the adjustment process despite their claims of independent credit analysis.

The next section describes the rating process for corporate borrowers and provides descriptive statistics of rating agency adjustments. Section III develops the hypotheses. Section IV describes the research design. Section V provides evidence of the construct validity of rating agency adjustments and reports the results of the empirical tests. Section VI concludes.

II. RATING PROCESS

This section first describes the economic role of rating agencies and the rating process for corporate borrowers. Next, I also provide empirical evidence on the scope, frequency, and magnitude of rating agency adjustments.

Economic Role of Rating Agencies

Rating agencies use accounting information from firms' financial statements to conduct credit risk assessments. However, firms have incentives to engage in earnings management and balance sheet management (Healy and Wahlen 1999). For example, firms attempt to avoid consolidation or recognition of off-balance sheet financing activity, such as operating leases, securitizations with recourse, and research and development (R&D) limited partnerships (Imhoff and Thomas 1988; Beatty et al. 1995; Engel, Erickson, and Maydew 1999; Mills and Newberry 2005). This allows them to report lower levels of debt on their balance sheets, resulting in lower leverage ratios, even if the information is disclosed elsewhere. Rating agencies analyze financial statements and make "analytical adjustments to better portray reality" and "to better reflect the underlying economics of transactions and events" (Standard & Poor's 2008, 22; Moody's 2006, 3). Under the view that rating agencies are efficient information intermediaries, bond investors rely on the rating agencies' reputations, and rating manuals sufficiently explain the types of adjustments rating analysts undertake (Klein and Leffler 1981; Shapiro 1983; Wakeman 1984; Partnoy 1999; White 2002; Securities and Exchange Commission [SEC] 2003; Strausz 2005). Reputational concerns provide powerful incentives for rating agencies to engage in high-quality information production.

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[3] Two related papers, De Franco, Wong, and Zhou (2011) and Batta, Ganguly, and Rosett (2014), provide more detailed descriptions of the individual categories of Moody's standardized quantitative adjustments and assess their associations with equity prices and bond yields, respectively. In contrast, this study investigates the implications for credit spreads and the credit spread term structure of the main adjustment for off-balance sheet finance, as well as soft and total adjustments.

[4] The literature on off-balance sheet finance activity has been limited by a lack of data. Researchers use Compustat data to capitalize operating leases (Imhoff, Lipe, and Wright 1993; Ely 1995), focus on one type of hand-collected off-balance sheet finance activity, such as securitizations (Gorton and Souleles 2006; Landsman, Peasnell, and Shakespeare 2008) or R&D development vehicles (Beatty, Berger, and Magliolo 1995), or analyze datasets of confidential tax returns (Mills and Newberry 2005).

However, the analysis of financial statements and the gathering of private information require unobservable effort, which results in a moral hazard problem (Leland and Pyle 1977; Gorton and Winton 2003). Debt investors litigate and monitor the rating agencies to mitigate this problem. Until 2010, the threat of litigation provided bond investors with limited recourse because rating agencies were protected under the First Amendment. Their ratings were considered opinions and, thus, courts have imposed a lower standard of care on rating agencies than on other information intermediaries such as accountants and auditors (Husisian 1990; Partnoy 2006).[5] Delegating information processing to an intermediary is rendered pointless because monitoring by individual debt investors is costly. Default is a rare event, so rating quality is observable only in some states of nature and often with a long time lag.[6] In addition, investors use ratings by certified rating agencies to comply with rating-based regulation, such as investment restrictions on what types of bonds they can hold in their portfolios. This is likely to give rise to regulatory arbitrage, in which investors derive benefits from ratings that are a function of the rating label as opposed to the informativeness of ratings. The prospect of regulatory arbitrage further distorts certified rating agencies' incentives (Partnoy 1999; C. Opp, M. Opp, and Harris 2013).[7] Finally, because rating agencies collect fees from the debt issuers they rate, they face a basic conflict between providing accurate ratings or upwardly biased ratings (Partnoy 1999; Mason and Rosner 2007; Becker and Milbourn 2011; Bolton, Freixas, and Shapiro 2012). Evidence on the ratings of structured finance products before and during the financial crisis is consistent with rating inflation (Mason and Rosner 2007; Benmelech and Dlugosz 2009). In summary, sufficient frictions exist that distort rating agencies' incentives to provide a comprehensive and neutral credit risk assessment.[8]

Rating Matrix for Corporate Bond Issuers

Moody's is one of the few certified rating agencies that provide credit ratings for corporate bond issuers.[9] Moody's classifies borrowers by industry and develops a matrix of quantitative factors and weights for each industry. The matrix generates a hypothetical rating that is the weighted average of all the quantitative factors. The factors include leverage ratios, profitability ratios, cash flow ratios, and firm size. Each borrower is evaluated on each of these quantitative dimensions. The hypothetical rating is calculated in two different versions. The first hypothetical rating is the weighted average of factors that are based on financial ratios calculated on the basis of U.S. GAAP reported numbers. The second hypothetical rating is the weighted average using financial ratios that include quantitative adjustments by the credit analysts. Hard adjustments include adjustments of reported numbers for operating leases, hybrid securities, securitizations, defined benefit pensions, capitalized interest, employee stock compensation, inventory valuations, and unusual and nonrecurring items (Moody's 2007).[10] The adjusted accounting numbers serve as input to the weighted average of the credit risk matrix.

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[5] Under the Dodd-Frank Wall Street Reform and Consumer Protection Act, rating agencies are subject to the same legal liability as auditors and security analysts (Goel and Thakor 2010). In response, certified rating agencies have refused to have their rating incorporated in public prospectuses for structured securities, citing legal liability concerns (van Duyn 2010). As of March 2014, no lawsuits had been filed against the major three credit rating agencies that exploit the new provisions of Dodd-Frank.

[6] For example, the historical probability of an investment grade-rated bond defaulting within three years is 0.780 percent, ranging from 0.000 percent for an AAA-rated bond to 1.186 percent for Baa-rated bond (Moody's 2009, Exhibit 46).

[7] Under regulatory arbitrage, ratings by certified rating agencies are valuable to regulated investors not for their information content, but to comply with regulation such as investment restrictions (Partnoy 1999; Opp et al. 2013).

[8] The current debate on rating structured finance products centers on whether rating inflation is due to active catering for business reasons or whether credit risk is underestimated because of erroneous judgments of non-traditional products (Coval, Jurek, and Stafford 2009; Ashcraft, Goldsmith-Pinkham, and Vickery 2010; Griffin and Tang 2012; He, Qian, and Strahan 2011).

[9] From 1975 until September 2007, only three to five rating agencies were certified as a full Nationally Recognized Statistical Rating Organization (NRSRO) at any given point in time. Since 2007, a few smaller rating agencies have received the NRSRO certification.

Appendix B illustrates the Moody's Financial Metrics rating process for 3M as of December 2007. Moody's rating analysts assign each industry group a rating grid that consists mainly of quantitative factors. For 3M, the rating grid captures assessments of its competitive position, size, stability, profitability, leverage, and financial strength. 3M's adjusted financials indicate that leverage is higher than that inferred from its reported financials. Debt-book capital and debt-EBITDA (earnings before interest, taxes, depreciation, and amortization) ratios calculated using adjusted financials are substantially greater than those calculated using reported financials and, thus, warrant lower ratings. Similarly, the cash flow to debt ratios calculated using adjusted financials are substantially lower than those calculated using reported financials and likewise warrant lower ratings. Overall, 3M's hypothetical rating on the basis of adjusted ratios is one notch lower than the rating that the reported financials imply.

In addition to hard adjustments, Moody's assesses qualitative factors of credit risk to make soft adjustments.[11] Soft adjustments incorporate factors such as management quality, aggressive accounting, weak controls, governance risk, industry structure, and managerial bondholder friendliness (Moody's 2007). The rating agency assigns a numerical score to this information and thereby "hardens" it. I infer the size of the soft adjustment (*SOFT*) from the difference between the hypothetical rating based on adjusted financial ratios and the actual rating that Moody's awards to the issuer, as illustrated in Appendix B. In the case of 3M, the soft adjustment offsets the rating decrease from hard adjustments because it improves the rating by one notch. Hence, the total adjustment (*TOTAL*) equals zero in this case. Appendix C provides a general illustration of the rating process.

Scope and Magnitude of Rating Agency's Adjustments

According to Moody's (2006) manual, financial statement adjustments include pensions, leases, hybrid securities, securitizations, capitalized interest, employee stock compensation, inventory valued at LIFO, and unusual and nonrecurring items. Most adjustments impact borrowers' leverage ratios. Moody's capitalizes operating leases and recognizes a related debt obligation. The agency also treats securitizations that do not fully transfer risk as collateralized borrowings. Any under- or unfunded portion of defined benefit pensions is treated as debt. Rating

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[10] Moody's capitalizes operating leases, treats securitizations that do not fully transfer risk as collateralized borrowings, and treats any under- or unfunded portion of defined benefit pensions as debt. Hybrids are reclassified and split into their debt and equity components, with weights assigned according to the hybrids' placement on Moody's debt-equity continuum classification scheme. Moody's de-recognizes capitalized interest by expensing it. Moody's expenses stock-based compensation and revalues LIFO (last in, first out) inventory on a FIFO (first in, first out) basis (Moody's 2006).

[11] My classification of adjustments into soft and hard categories follows Petersen (2004). Hard information can be reduced to numbers and is easy to transmit. Moody's adjustments to reported financial numbers fall into this category, as they capture its credit analysts' assessments of credit risk stemming from quantitative risk factors, such as the magnitude of off-balance sheet debt (Moody's 2006). Moody's assesses credit risk stemming from qualitative risk factors, which I label soft adjustments.

TABLE 1

Summary Statistics

Variable	Mean	p25	p50	p75	n
Firm Characteristics					
Total Assets	13,301	1,869	4,467	13,200	1,729
Revenues	11,980	1,438	4,041	10,594	1,729
Leverage	0.33	0.19	0.29	0.42	1,729
Coverage	11.00	1.90	4.90	12.00	1,729
Operating Margin	0.12	0.05	0.10	0.17	1,729
ROA	0.09	0.05	0.09	0.13	1,729
Tangibility	0.49	0.33	0.49	0.65	1,729
Rating	10.0	8.0	10.0	13.0	1,729
Audit_Length	14.0	6.0	10.0	19.0	817
Numrec	12.0	6.0	10.0	17.0	817
Gindex	9.7	8.0	10.0	12.0	817
Credit Risk in CDS Sample					
Rating	9.7	8.0	9.0	12.0	477
Spread5y (bsp)	170.0	41.0	81.0	190.0	477
SpreadRatio	0.71	0.55	0.72	0.84	366
Rating Agency Adjustments					
OFFBSD	0.14	0.04	0.08	0.15	1,729
SOFT	0.38	−1.00	0.00	1.00	1,729
TOTAL	0.83	0.00	1.00	2.00	1,729

The table reports summary statistics. All observations have required information on *OFFBSD*, *SOFT*, and *TOTAL*. The CDS sample is a subset of this sample and has required information on *Spread5y*.
Variables are defined in Appendix A.

analysts split hybrid securities, such as preferred stock or convertible debt, into their debt and equity components, with weights assigned according to the hybrids' placement on Moody's debt-equity continuum classification scheme. Capitalized interest is added to the reported interest expense. Finally, Moody's revalues LIFO inventory on a FIFO basis.[12]

I calculate Moody's adjustments to financial statement items as the net line-by-line differences in reported and adjusted balance sheets, income statements, and cash flow statements. I collect reported and adjusted financial statements from Moody's Financial Metrics from 2002 until 2008 for U.S.-domiciled nonfinancial issuers.[13] This detailed information is available for a sample of 1,737 issuers and comprises 8,117 firm-year observations. The hypothetical ratings are available for a subset of these observations only. After imposing the requirement to have the two hypothetical ratings in addition to the actual rating, the sample consists of 849 issuers and 1,729 firm-year observations. This subset is representative of the total sample from Financial Metrics in that firms have similar size, leverage, profitability, and creditworthiness.

Table 1 reports summary statistics for the main variables used in the analysis. On average, firms have revenues of US$13 million, leverage of 33 percent, and return on assets of 9 percent. The average firm has a rating of BBB– (numeric value of 10).[14] On average, a firm has retained the same auditor for 14 years, is followed by 12 equity analysts, and receives a G-Score of 9.7. Data on the length of the relationship with the auditor are provided by Compustat and the number of equity analysts is calculated based on I/B/E/S. The G-Score, an index evaluating the firm's corporate governance, is obtained from RiskMetrics and follows the methodology in Gompers, Ishii, and Metrick (2003). The average five-year CDS spread (*Spread5y*) is 170 basis points. I obtain CDS spreads from the Markit database, which covers a majority of CDS contracts written on U.S.-based entities.[15] Out of 1,737 issuers in the total sample, 578 can be identified in the Markit database. Information on *Spread5y,* as well as the two hypothetical ratings, is available for 477 firm-years (299 issuers), which I refer to as the *CDS sample.*

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[12] Moody's adjusts inventory recorded on a LIFO basis to FIFO on the balance sheet, but does not adjust cost of goods sold.

[13] I exclude a few observations with zero reported revenues, as well as those classified as financial conduits and captive finance companies, because the traditional measures of leverage do not apply.

The rating agency estimate of off-balance sheet debt, *OFFBSD,* amounts to 14 percent of total assets, on average. *SOFT* lowers the rating by 0.38 notches and *TOTAL* by 0.83 notches.[16]

Credit analysts make extensive adjustments in their credit risk analysis. Table 2 documents the extent of the rating agency's adjustments to financial statements and the impact of those adjustments on leverage, profitability, and cash flow ratios.[17] Table 2, Panel A reports scaled net adjustments, that is, differences between as-reported and as-adjusted amounts of selected accounts divided by total reported assets. In addition, Panel A reports the frequency of adjustments as a proportion of firm-year observations for the selected accounts. Overall, these findings provide new evidence on the widespread extent and magnitude of the credit rating agency's adjustments for borrowers' off-balance sheet debt. For 98 percent of the sample, long-term debt increases as a result of the recognition of off-balance sheet debt and the reclassification within the balance sheet of on-balance sheet hybrids. The median (average) increase for net long-term debt amounts to 7 percent (13 percent) of total assets, despite the fact that some long-term debt is reclassified as short-term debt. The median (average) increase of total liabilities amounts to 5 percent (11 percent) of total assets, which is primarily caused by increases in net long-term debt and recognition of obligations from operating leases. The average effects are greater than the median effects because for a number of firms, the adjustments are substantial.

Compared with the adjustments to debt and liabilities in Table 2, Panel A, the impact on shareholders' equity is relatively small. Although shareholders' equity decreases for 43 percent of the sample, the adjustment is small, with the median change in shareholders' equity amounting to 0 percent of total assets. For 92 percent of all firm-year observations, total assets are increased by the adjustments, mainly due to the recognition of additional property, plant, and equipment (PPE). The median (average) increase in total assets is 5 percent (11 percent). PPE is adjusted upward for 97 percent of all observations. Inventory and accounts receivable are adjusted upward for 16 percent and 12 percent of the sample, respectively, as a result of inventory revaluation and the reversing of securitizations with recourse.[18] The frequency and magnitude of the untabulated adjustments to goodwill and other intangibles are negligible.

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[14] The rating is converted into numerical values from 1 (AAA) to 21 (C). Higher numerical values denote lower credit ratings.

[15] Typically, Markit reports a composite daily CDS spread that is an average across the quotes provided by all market makers after removing outlying observations. This study focuses on five-year maturity contracts because they represent the most liquid contracts across different maturities. To maintain contractual uniformity, I follow prior literature (Jorion and Zhang 2009; Zhang, Zhou, and Zhu 2009) and only keep spreads for CDS contracts referencing senior debt with modified restructuring (MR) clauses and denominated in U.S. dollars. MR clauses are one of four types of restructuring clauses in CDS contracts that specify whether restructuring triggers a credit event and put limitations on the terms of bonds that can be delivered. MR clauses are the market standard in the U.S. over the sample period.

[16] One notch is the difference between two consecutive ratings; for example, the difference between A+ and A.

[17] All the descriptions of detailed adjustments apply to the total sample of firms, as well as the subset with available hypothetical ratings. I restrict my discussion to the subset.

[18] Note that adjustments for securitizations only affect the balance sheet, but not net income because gains on sale from securitizations with recourse are not automatically reversed in the income statement.

TABLE 2
Description of Rating Agency Adjustments

Panel A: Rating Agency's Adjustments to Reported Financial Statements

	Adjustment in Percent of Total Assets				Frequency of Adjustments		
Account	**Mean**	**p25**	**p50**	**p75**	**Total**	**Up**	**Down**
Adjustments to Assets							
A/C Receivable – trade (net)	0.4%	0.0%	0.0%	0.0%	12.0%	12.0%	0.0%
Inventories	0.4%	0.0%	0.0%	0.0%	18.0%	16.0%	1.8%
PPE (gross)	11.0%	2.3%	4.9%	9.8%	99.0%	97.0%	1.9%
Total assets	11.0%	1.8%	5.0%	11.0%	99.0%	92.0%	7.3%
Adjustments to L&SE							
Capitalized lease obligation	11.0%	2.3%	5.0%	9.8%	98.0%	98.0%	0.0%
Long-term debt (gross)	14.0%	3.7%	7.6%	14.0%	99.0%	98.0%	1.4%
Current portion of long-term debt	−0.9%	−0.9%	−0.4%	−0.2%	95.0%	0.0%	95.0%
Long-term debt (net)	13.0%	3.5%	7.2%	13.0%	99.0%	98.0%	1.6%
Total liabilities	11.0%	2.3%	5.3%	11.0%	99.0%	94.0%	4.7%
Shareholders' equity	−0.5%	−0.5%	0.0%	0.0%	65.0%	22.0%	43.0%
Adjustments to Income Statement							
Revenues	0.0%	0.0%	0.0%	0.0%	1.1%	0.6%	0.5%
Gross profit	1.4%	0.0%	0.6%	1.7%	81.0%	77.0%	3.4%
Operating profits	1.0%	0.1%	0.4%	1.1%	99.0%	80.0%	19.0%
Pre-tax income	0.2%	−0.5%	−0.1%	0.2%	91.0%	35.0%	56.0%
Net income	0.3%	−0.3%	0.0%	0.2%	91.0%	37.0%	54.0%
Adjustments to Cash Flows							
CFO	1.3%	0.2%	0.7%	1.7%	99.0%	86.0%	13.0%
CFI	−1.1%	−1.1%	−0.5%	−0.2%	99.0%	10.0%	88.0%
CFF	−0.1%	−0.2%	0.0%	0.0%	53.0%	12.0%	41.0%

Panel A reports the detailed adjustments to financial statement line items. Adjustment is the difference between the adjusted and reported account. Adjustment in percent of total assets is the ratio of adjustment to total reported assets. Total frequency of adjustments is the number of observations with a non-zero net adjustment divided by the total number of observations. Up (Down) Frequency is the number of observations with a positive (negative) adjustment divided by the total number of observations. CFO is operating cash flow. CFI is investing cash flow. CFF is financing cash flow.

Panel B: Impact on Selected Financial Ratios

Percentage Change in Ratios	**Mean**	**p50**	**p10**	**p25**	**p75**	**p90**
Change in Leverage Ratios						
Total debt/total assets	69.7%	20.1%	1.9%	7.1%	47.7%	99.9%
Total liabilities/total assets	5.4%	2.9%	−0.3%	0.9%	7.0%	14.3%
Change in Coverage Ratio						
Coverage	−27.2%	−19.5%	−66.8%	−41.0%	−6.2%	8.8%
Change in Profitability Ratios						
ROA	−9.0%	−1.7%	−24.4%	−8.1%	2.4%	19.6%
Operating Margin	1.3%	3.4%	−10.9%	0.0%	11.3%	36.1%
Change in Cash Flow Debt Ratios						
CFO/Debt	−8.3%	−14.6%	−55.0%	−33.3%	−4.5%	5.5%
CFI/Debt	−12.6%	−13.6%	−56.4%	−31.9%	−3.5%	6.8%
FCF/Debt	−25.4%	−20.5%	−67.6%	−41.3%	−8.0%	−1.1%

(continued on next page)

TABLE 2 (continued)

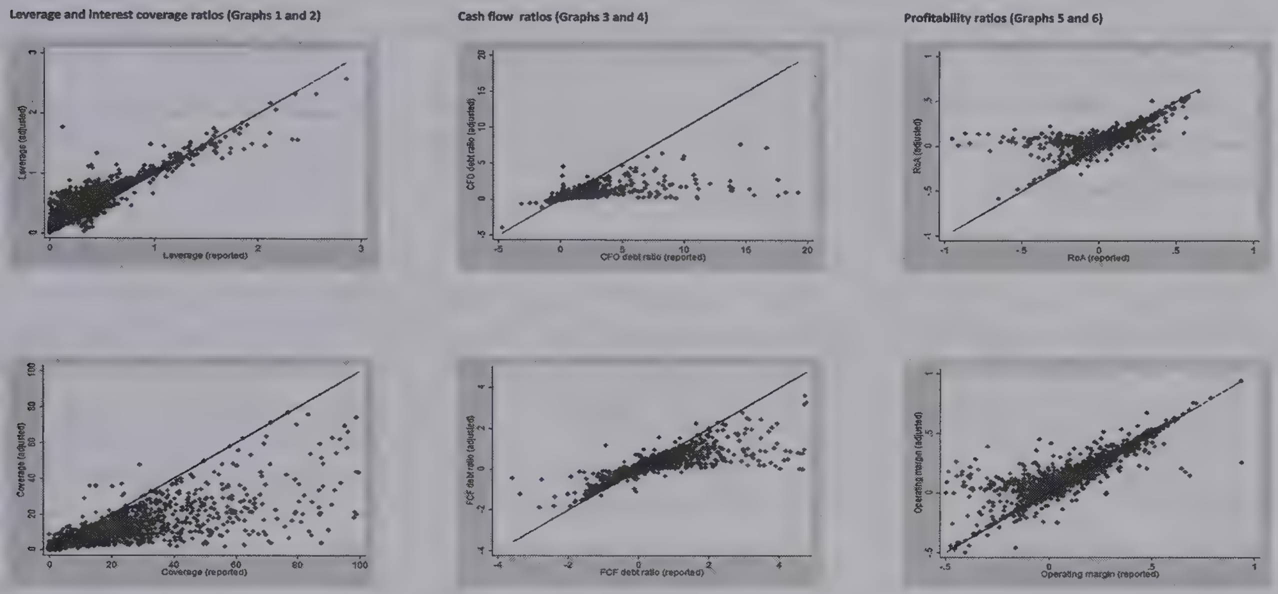
Panel B reports the impact on selected financial ratios. The leverage ratio is the ratio of debt to total assets. The coverage ratio is the ratio of EBIT to total interest expense. EBIT is pre-tax income plus interest expense plus other nonrecurring expenses (gains). ROA is the ratio of operating profit to total assets. Operating Margin is the ratio of operating profit to revenues. Cash flow debt ratio is the ratio of debt to total debt. CFO is operating cash flow. CFI is investing cash flow. CFF is financing cash flow. FCF is free cash flow, which equals the sum of CFO and CFI. Impact on ratios is calculated as (adjusted ratio/reported ratio) − 1.

Panel C: Adjustments by Industry

			Percentage of Upward Adjustments		
Peer Group	**Impact on Leverage Ratio Mean**	***OFFBSD* Mean**	**Capitalized Lease Obligation**	**Long-Term Debt (Gross)**	**n**
Aerospace/Defense	53.8%	10.6%	100.0%	100.0%	53
Agriculture	24.4%	12.9%	100.0%	100.0%	28
Alcoholic Beverage	10.5%	5.0%	91.7%	100.0%	24
Apparel	71.1%	28.1%	100.0%	100.0%	28
Automotive	70.5%	12.1%	91.4%	91.4%	35
Building Materials	22.6%	9.6%	100.0%	100.0%	31
Chemicals	43.0%	11.9%	100.0%	100.0%	153
Construction	87.2%	11.8%	100.0%	100.0%	7
Consumer Products	39.4%	11.7%	100.0%	100.0%	147
Electric Utilities	3.4%	5.8%	100.0%	100.0%	1
Energy	25.2%	5.9%	99.2%	100.0%	256
Forest Products	30.7%	7.3%	98.2%	100.0%	111
Gaming/Lodging	8.3%	13.3%	100.0%	100.0%	82
Healthcare	155.7%	9.7%	100.0%	100.0%	55
Homebuilding	10.3%	4.2%	100.0%	100.0%	26
Manufacturing	61.4%	11.5%	100.0%	100.0%	79
Media	−2.1%	2.8%	100.0%	100.0%	6
Metals, Mining, and Steel	30.4%	5.1%	85.0%	95.0%	120
Packaging	18.1%	12.4%	100.0%	100.0%	58
Pharmaceutical	270.8%	6.1%	100.0%	100.0%	66
Retail	248.9%	58.6%	100.0%	100.0%	137
Services	194.4%	16.7%	100.0%	100.0%	66
Shipping	46.7%	43.7%	100.0%	100.0%	25
Soft Beverage	11.1%	7.1%	100.0%	100.0%	10
Technology	56.2%	5.0%	100.0%	100.0%	31
Telecommunications	23.1%	8.9%	94.3%	97.1%	70
Transportation Services	8.6%	12.8%	100.0%	100.0%	7
Tobacco	22.2%	3.9%	100.0%	100.0%	1
Waste Management	3.5%	3.8%	100.0%	100.0%	12
Wholesale Distribution	45.4%	21.4%	100.0%	100.0%	4

Panel C reports adjustments for leases and debt by Moody's industry classification. *OFFBSD* equals the rating agency estimate of off-balance sheet debt divided by total assets.

FIGURE 1
Scatter Plots of Reported versus Adjusted Leverage and Profitability Ratios



The leverage ratio is the ratio of total debt to total assets. Total debt is the sum of long-term and short-term debt. The coverage ratio is the ratio of EBIT to total interest expense. ROA is the ratio of operating profit to total assets. Operating Margin is the ratio of operating profit to revenues. Cash flow debt ratio is the ratio of debt to total debt. CFO is operating cash flow. FCF is free cash flow that equals the sum of operating cash flow and investing cash flow. Reported indicates the ratio is calculated from amounts as reported in the financial statements. Adjusted indicates the ratio is calculated from amounts as adjusted by the rating agency. The 45 degree line is shown for reference.

Due to the recognition of additional debt, the rating agency reclassifies certain operating expenses as interest expense and depreciates adjusted PPE. As a result of the reclassification, gross profit and operating profit increase for 77 percent and 80 percent of all observations, respectively, in Table 2, Panel A, but pre-tax income decreases for 56 percent of the sample. In terms of the bottom line, net income is adjusted downward (upward) for 54 percent (37 percent) of the sample. For most firms, the capitalization of operating leases leads to increases in operating cash flows and decreases in investing cash flows, which reflects the reclassification of the principal portion of rent expense as non-operating and the recognition of capital expenditures for assets under operating leases, respectively.

The rating agency's adjustments to financial statements significantly impact leverage, coverage, and cash flow ratios. Table 2, Panel B shows that leverage and coverage ratios reflect higher levels of indebtedness as a result of the rating agency's adjustments. The ratio of total debt divided by total assets increases on average (median) by 70 percent (20 percent). The coverage ratio decreases on average (median) by 27 percent (20 percent). Figure 1, Graphs 1 and 2 present scatter plots of leverage and interest coverage, respectively, with reported ratios on the horizontal axis and adjusted ratios on the vertical axis. Most observations are above (below) the 45-degree line; with a few exceptions, the adjusted leverage ratios exceed reported leverage ratios, and *vice versa* for the coverage ratio. If the rating agency's adjustments capture off-balance sheet financing, then a significant number of firms understate their leverage based on reported book values. Furthermore, the cash flow to debt ratios deteriorate as well, due to the overall increase of indebtedness, as shown in Figure 1, Graphs 3 and 4. The ratios of Cash Flow from Operations (CFO) to debt and Cash Flow from Investing (CFI) to debt deteriorate for more than 75 percent of all firms, and the ratio of Free Cash Flow (FCF) to debt deteriorates for more than 90 percent of all firms in the sample. In contrast, the impact of adjustments on return on assets and operating margin are more symmetrical, as shown in Figure 1, Graphs 5 and 6. The median change in return on assets (operating margin) amounts to only −2 percent (3.4 percent). A vast majority of the adjustments increase the amount of debt and interest expense as the result of the recognition of additional debt and the reclassification of certain hybrid securities as completely or partially debt.[19]

Table 2, Panel C reports the impact, magnitude, and frequency of Moody's adjustments by Moody's industry classification.[20] Energy, chemicals, and consumer products are the largest industries in the sample. Across all industries, long-term debt is adjusted upward for more than 91 percent of all observations and, in many cases, for every firm within an industry. Similarly, Moody's capitalizes operating leases for more than 91 percent of all observations within an industry and, in many cases, for all firms within an industry. Across all industries, a major contributor to *OFFBSD* is the capitalization of operating leases. The estimates of *OFFBSD* range from 58.6 percent for retail to 2.8 percent for media, which reflects different industry characteristics, such as asset tangibility. The leverage ratios in all industries except for media increase.[21] Under the proposed accounting rules on lease accounting, as discussed in the joint project between FASB and IASB (FASB 2013), leverage ratios are likely to increase substantially unless firms restructure their financings (Imhoff and Thomas 1988).

III. HYPOTHESES DEVELOPMENT

The first hypothesis pertains to whether rating agency adjustments capture credit risk. Greater leverage is associated with greater risk (Merton 1974). Under the assumption of market efficiency, greater leverage should be reflected in greater credit spreads, regardless of whether the additional debt is recognized on the face of the balance sheet or whether it is disclosed in the footnotes (Bernard and Schipper 1994). If the rating agency provides an accurate estimate of credit risk based on its adjustments, then its adjustments should be associated with the market's assessment of credit risk. If there is no such association, then the adjustments do not properly capture credit risk. Thus, I examine whether rating agency adjustments are priced by debt market participants.

The market's assessment of default risk, such as CDS spreads or bond yields, serves as a reasonable benchmark of credit risk because I assume that the market takes into account rating agencies' biases. For example, AAA-rated structured finance securities were traded at much higher yields than AAA-rated corporate bonds because investors price-protected against inflated ratings for structured finance securities (JPMorgan 2006; Adelino 2009). Studies on stock price are consistent with the interpretation that stock prices reflect unrecognized, but disclosed, information. For example, the market price reflects the distinction between securitizations with risk transfer and those without risk transfer (Landsman et al. 2008). Incorporating unrecognized, but disclosed, liabilities improves explanations of risk with respect to disclosures on leasing activity (Bowman 1980; Imhoff et al. 1993; Ely 1995; Lim, Mann, and Mihov 2003; Altamuro, Johnston, Pandit, and Zhang 2014) and pensions (Dhaliwal 1986).[22]

Consistent with the results from the recognition versus disclosure literature, I predict that rating agency adjustments capture credit risk and that they are associated with greater credit spreads. However, it is possible that this relation does not hold, for example, because several frictions in the credit rating agency industry may exacerbate the moral hazard problem. Regulatory arbitrage based on rating labels and weak discipline-inducing mechanisms may also distort the rating agency's incentives. Even hard adjustments based on disclosures in firms' reports, such as the footnotes, involve a certain degree of discretion. The credit analyst must still estimate the discount rate to calculate the present value of future payouts or estimate the magnitude of the rent expense multiplier. Furthermore, soft adjustments may not effectively capture credit risk. Because they are difficult to verify by outside observers or in court, such adjustments might be used to reverse the impact of adverse hard adjustments. In that case, soft adjustments will not be empirically associated with greater credit spreads. I test the following hypothesis:

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[19] For example, when preferred stock is treated as debt, there is no change in total assets.

[20] The total sample has a greater number of industries, but the results are the same as for the subset.

[21] In untabulated results, media firms' leverage ratios increase when sample size increases.

[22] In contrast, rating and bond yield models in the finance literature generally rely on issuers' reported GAAP numbers and ignore adjustments to balance sheet debt (Kaplan and Urwitz 1979; Blume, Lim, and Mackinlay 1998; Campbell and Taksler 2003; Chen, Lesmond, and Wei 2007).

H1: The rating agency adjustments are associated with greater credit spreads.

My second hypothesis addresses whether greater rating agency adjustments imply greater uncertainty about the firm and, thus, make it harder for debt investors to assess the value of the firm's assets and the risk of default. Duffie and Lando (2001) predict that the quality of a firm's information disclosure can affect the term structure of its credit spreads. More specifically, they suggest that proxies of asset measurement uncertainty should be positively related with short-term credit spreads and also with the relative flatness of the credit term structure. Using an asset pricing model of default risk, Duffie and Lando (2001) show theoretically that sufficiently short-term credit spreads would equal zero absent asset value measurement uncertainty, because some passage of time would be necessary for any possibility of default to arise. Asset value measurement uncertainty makes debt investors uncertain about how close the firm is to default. With sufficient uncertainty, default is possible over any finite interval. This raises short-term credit spreads relative to longer-term spreads, flattening the credit spread term structure. Intuitively, as the term lengthens, the importance of asset value measurement uncertainty decreases relative to economic uncertainty.

Yu (2005) and Arora et al. (2011) provide empirical support for Duffie and Lando's (2001) theoretical result. Yu (2005) uses firms' Association for Investment Management and Research (AIMR) disclosure rankings as proxies for firms' asset value measurement uncertainty. The study finds evidence of a transparency component in credit spreads, which is especially pronounced at short-term maturities. More specifically, worse AIMR disclosure rankings result in a flatter term structure yield curve. Arora et al. (2011) use the magnitude of firms' Statement of Financial Accounting Standards (SFAS) 157 Level 2 and 3 financial assets as proxies for firms' asset value measurement uncertainty. This study also finds that greater uncertainty is associated with flatter credit spread term structures.

Building on these term structure results, I predict that rating agency adjustments are associated with flatter term structures. Following Arora et al. (2011), I measure the steepness of the credit spread term structure as the ratio of a short-maturity CDS spread to a long-maturity CDS spread. I calculate the ratio of the five- to 20-year CDS spreads (*SpreadRatio*) as a proxy for the steepness of the term structure. I predict that to the extent that greater rating agency adjustments imply greater uncertainty about the firm and, thus, make it harder for debt investors to assess the risk of default, the adjustments are associated with greater short-term credit spreads relative to long-term credit spreads. I test the following hypothesis:

H2: Rating agency adjustments are positively associated with the ratio of the five-year CDS spread to the 20-year CDS credit spread, *ceteris paribus*.

IV. RESEARCH DESIGN

To test my hypotheses, I assess the association between rating agency adjustments and proxies for credit risk and the credit risk term structure. This section describes the proxies for firms' credit risk, as well as the control variables. I also develop the empirical models to test the hypotheses and to conduct the validation analysis of rating agency adjustments.

Validation of Rating Agency Adjustments

This subsection provides evidence on the validity of rating agency adjustments. I expect greater rating agency adjustments to capture greater credit risk from greater leverage due to off-balance sheet finance. Hence, I formally test whether the major line-by-line adjustments are associated with lower ratings. Following the credit rating prediction literature, I estimate the default risk model with the following specifications.[23] I regress *Rating* on credit rating agency adjustments, *ADJ*, and control variables. The dependent variable, *Rating*, is Moody's long-term issuer rating on the filing date, converted into numerical values from 1 (AAA) to 21 (C), where higher numerical values of *Rating* denote lower credit ratings:

$$Rating_{i,t} = \alpha + \beta ADJ_{i,t} + \sum_{n} \gamma_n Controls_{n,i,t} + \varepsilon_{i,t}. \tag{1}$$

In variable subscripts, *i* denotes firms and *t* denotes time. The variable of interest is *ADJ*, which is either *OFFBSD*, *SOFT*, or *TOTAL* in the main tests. *OFFBSD* is the net adjustment to total debt as reported on the balance sheet, scaled by total assets. *SOFT* is the difference between the actual rating and the hypothetical rating based on adjusted financials. *TOTAL* is the difference between the actual rating and the hypothetical rating based on reported financials. Naturally, in this validation exercise, I expect higher levels of *OFFBSD*, *SOFT*, and *TOTAL* to be associated with lower ratings. In one specification, I use both *OFFBSD* and *SOFT* to test whether one subsumes the effect of the other. I expect both of them to be associated with *Rating* because I predict hard and soft adjustment to capture different dimensions of credit risk. In additional tests, I replace the main variable of interest with various alternative measures of hard adjustments, including adjustments to capitalized operating leases, adjustments to cash flows, and adjustments to profits. To the extent that these adjustments are determined primarily by off-balance sheet debt, I would expect them to be associated with *Rating*, as well.

Controls for firm characteristics (*Controls*) include firm size (logarithm of revenues, denoted *LN_Rev*), profitability (return on assets, denoted *ROA*), book-leverage (*Leverage*), and asset tangibility (*Tangibility*). These controls are based on reported U.S. GAAP amounts. In some models, I control for the number of equity analysts (*Numrec*), the length of the firm's relationship with its auditor (*Audit_Length*), and an index evaluating the firm's corporate governance, the G-Score (*Gindex*), although this entails a loss of observations. Mansi, Maxwell, and Miller (2004) show that auditor tenure is negatively associated with cost of debt financing. Cheng and Subramanyam (2008) find that the number of equity analysts is negatively associated with credit risk. Ashbaugh-Skaife, Collins, and LaFond (2006) document that weaker corporate governance is associated with lower ratings. Hence, I expect negative coefficients for *Audit_Length*, *Numrec*, and *Gindex*.

I use fixed effects for regulated industries and year fixed effects to control for changes in the macroeconomic environment.[24] I estimate the model using ordinary least squares (OLS), and because

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[23] Early papers in the credit rating literature employ linear regression and discriminant analysis, whereas later papers use an ordered probit approach. In general, firm-specific variables include financial ratios measuring profitability, leverage, and interest coverage, as well as size. Later models include measures of equity risk (market beta and unsystematic risk). Rating predictions models are estimated in Horrigan (1966), West (1970), Pogue and Soldofsky (1969), Pinches and Mingo (1973), Kaplan and Urwitz (1979), Ederington (1985), and Blume et al. (1998).

[24] I exclude other control variables, such as equity beta or equity volatility, because the objective is to test for the association between adjustments and default risk, rather than to maximize the explanatory power of the default risk model *per se*. These variables are likely to be a function of the off-balance sheet debt estimates.

Rating is a categorical variable, I also employ ordered probit regressions (Ederington 1985). If the error term in the equation with the latent continuous dependent variable is normally distributed, then the ordered probit specification is theoretically more appropriate. However, ordered probit estimates are inconsistent if this error term is not normal, and OLS estimates may be more robust to this and other model specification issues. Standard errors in all regressions in the paper are clustered by firm and year.

Relation between Rating Agency Adjustments and Credit Spreads

To test the first hypothesis, I assess the association between rating agency adjustments and credit spreads. I follow the methodology of recent models of credit spreads including CDS spreads and bond yields, such as Campbell and Taksler (2003) and Chen et al. (2007), which build on results from the rating prediction literature; in particular, on the rating model in Blume et al. (1998). Following this literature and to be consistent with the validation model in the prior subsection, I estimate the following model in which the dependent variable, *Spr5y,* is the logarithm of the five-year CDS spread:

$$Spr5y_{i,t} = \alpha + \beta ADJ_{i,t} + \sum_n \gamma_n Controls_{n,i,t} + \varepsilon_{i,t}. \tag{2}$$

In variable subscripts, *i* denotes firms and *t* denotes time. The variable of interest, *ADJ,* is either *OFFBSD, SOFT,* or *TOTAL,* which are defined in the prior subsection. In one specification, I include both *OFFBSD* and *SOFT* to test whether one subsumes the other. Controls for firm characteristics are measured at fiscal year-end and are similarly defined above. Under H1, I expect *Spr5y* to have a positive association with *OFFBSD, SOFT,* and *TOTAL.* I expect *Spr5y* to be an increasing function of business risk and leverage (Merton 1974). In line with prior literature, I expect *LN_Rev* and *ROA* (*Leverage*) to have a negative (positive) association with *Spr5y.* In some specifications, I include additional controls, which yields a smaller sample. I expect negative coefficients for *Audit_Length, Numrec,* and *Gindex.* Fixed effects for regulated industries and year fixed effects are included. Standard errors are clustered by firm and year.

As described in the prior subsection, I include alternative measures of hard adjustments, specifically, adjustments to capitalized operating leases, adjustments to cash flows, and adjustments to profits, in additional tests. I expect adjustments to *Capitalized Leases* to be positively associated with *Spr5y.* Adjustments to gross profit primarily reflect the reclassification of operating expense to interest expense due to the capitalization of leases and, thus, I expect a positive association with *Spr5y.* Similarly, I expect adjustments to operating cash flow (*CFO*) and adjustments to investing cash flow (*CFI*) to have positive and negative associations with *Spr5y,* respectively. As described in Section II, the capitalization of operating leases leads to increases in operating cash flows and decreases in investing cash flows, because the principal portion of rent expense is reclassified as non-operating, and capital expenditures for assets under operating leases are reclassified as financing.

As an additional formal test, I conduct a misspecification test using a non-nested J-test (Davidson and MacKinnon 1981; Maddala 2001). Basically, I run a specification race between a credit risk model using financial ratios that incorporate reported accounting numbers (*REP*), and another model using financial ratios that incorporate rating agency adjustments (*ADJ*). While both sets of regressors explain variations in credit risk, the J-test helps to discriminate between these alternative specifications. The two specifications are:

$$ADJ\colon\ DefaultRisk = \lambda AdjustedFirmChar + \zeta SOFT + \tau TOTAL + \delta Controls + \varepsilon. \tag{3}$$

$$REP\colon\ DefaultRisk = \lambda ReportedFirmChar + \delta Controls + \varepsilon. \tag{4}$$

Firm characteristics (*FirmChar*) include size, profitability, coverage, leverage, and tangibility. *ReportedFirmChar* refers to those characteristics measured by accounting ratios based on numbers as recognized in financial statements, whereas *AdjustedFirmChar* refers to those accounting numbers based on the numbers as adjusted by the rating agency. To test whether financials adjusted by the rating agency is the better default risk model, the predicted default risk $(\widehat{DefaultRisk})$ is estimated by running the alternative specification, the *REP* model, which is then included as an additional explanatory variable in the estimation of the *ADJ* model. I expect the default risk model based on adjusted ratios to outperform the model that is based on reported ratios.

Relation between Rating Agency Adjustments and Credit Spread Term Structures

H2 predicts that rating agency adjustments will be associated with flatter term structures. I measure the steepness of the credit spread term structure as the ratio of a short-maturity CDS spread to a long-maturity CDS spread. I calculate the ratio of the five- to 20-year CDS spreads (*SpreadRatio*) as a proxy for the steepness of the term structure. To test H2, I regress *SpreadRatio* on *ADJ* and the same set of control variables as in Equation (2):

$$SpreadRatio_{i,t} = \alpha + \beta ADJ_{i,t} + \sum_{n} \gamma_n Controls_{n,i,t} + \varepsilon_{i,t}. \tag{5}$$

V. EMPIRICAL RESULTS

Table 3 reports the pairwise Pearson correlations for the key variables. *Spr5y* and *Rating* are highly correlated with a correlation coefficient of 0.87. Consistent with the prior discussion, rating agency adjustments *OFFBSD, SOFT,* and *TOTAL* are all significantly correlated with greater values of *Rating, Spr5y,* and *SpreadRatio.* All of these correlations are significant at the $p < 0.05$ level. The univariate correlations of the control variables with *Rating* and *Spr5y* have the expected signs. Greater profitability and size are correlated with higher ratings and lower credit spreads. Greater leverage based on book debt is correlated with lower ratings and higher credit spreads. Greater auditor tenure, greater number of equity analysts, and greater values of G-Score are correlated with higher ratings and lower credit spreads.

Furthermore, in Table 3, I find that adjustments to the obligation from capitalized operating leases are significantly correlated with lower ratings and higher credit spreads. Increases in operating cash flow (*CFO*), decreases in investing cash flow (*CFI*), and increases in *Gross Profit* arising from rating agency adjustments are significantly associated with higher credit spreads, indirectly reflecting the impact of off-balance sheet debt adjustments. The rating agency's total adjustments are significantly and highly correlated with adjustments to book-debt, operating cash flow, investing cash flow, free cash flow, and gross profit. The adjustments to *CFO* and *CFI,* as well as those to *Gross Profit,* largely reflect the recognition of off-balance sheet debt. The correlations between soft adjustments and adjustments to financial statements for *Capitalized Leases, CFO,* and *CFI* are also significant, but are substantially smaller, which suggests that soft adjustments capture other factors than the amount of off-balance sheet debt.

First, I empirically validate the various measures of rating agency adjustments by formally testing whether they are associated with lower ratings. Table 4, Panel A reports the results of estimating Equation (1) with *Rating* as the dependent variable and the three main adjustments—*OFFBSD, SOFT,* and *TOTAL*—as the main variables of interest. Columns 1–4 (Columns 5–8) of Panel A report the results excluding (including) *Audit_Length, Numrec,* and *Gindex,* which yields a larger (smaller) sample. The coefficients on *OFFBSD, SOFT,* and *TOTAL* are significantly positive in all columns of the panel, demonstrating the robustness of the results in this validation analysis. Using the results of the OLS regression for the larger sample reported in Column 1, a one-standard-deviation increase in *OFFBSD* is associated with an increase of 4.5 percent of the average rating. Using the results in Column 2 (Column 3), a one-standard-deviation increase in *SOFT* (*TOTAL*) is

TABLE 3

Pairwise Correlations

Panel A: Correlation Variables *Rating* to *ADJ to CFO*

	Rating	*LN_Spr5y*	*SpreadRatio*	*OFFBSD*	*SOFT*	*TOTAL*	*Capitalized Leases*	*Adj. to Gross Profit*	*Adj. to CFO*
Rating	1.0000								
LN_Spr5y	0.8748*	1.0000							
SpreadRatio	0.4846*	0.7876*	1.0000						
OFFBSD	0.1228*	0.1841*	0.0576*	1.0000					
SOFT	0.3338*	0.2736*	0.1080*	0.0182*	1.0000				
TOTAL	0.2524*	0.1909*	0.0571*	0.1339*	0.9038*	1.0000			
Capitalized Leases	0.1268*	0.1864*	0.0636*	0.9688*	0.0178*	0.1230*	1.0000		
Adj. to Gross Profit	0.0098	0.1997*	0.0767*	0.5738*	0.0104	0.0883*	0.5632*	1.0000	
Adj. to CFO	0.0414*	0.1441*	0.0374*	0.5481*	0.0185*	0.0554*	0.5674*	0.3464*	1.0000
Adj. to CFI	−0.1173*	−0.1322*	−0.0597*	−0.7197*	−0.0284*	−0.0757*	−0.7433*	−0.3867*	−0.5051*
Adj. to FCF	−0.0871*	−0.0256	0.0181	−0.2186*	−0.0116	−0.0252*	−0.2245*	−0.0673*	0.4412*
LN_Rev	−0.6866*	−0.2881*	−0.1325*	−0.0217*	−0.1087*	−0.0214*	−0.0422*	0.0985*	−0.0016
Leverage	0.5516*	0.4357*	0.2247*	0.0606*	0.0068	−0.1014*	0.0689*	−0.0441*	0.0828*
ROA	−0.3025*	−0.4194*	−0.3657*	0.0210*	0.2357*	0.2579*	0.0467*	0.0159	0.0115
Tangibility	0.1656*	0.0634*	0.0318	0.0224*	−0.1167*	−0.1239*	0.0272*	−0.0016	−0.0480*
Audit_Length	−0.2545*	−0.1785*	−0.0672*	0.0196*	−0.0860*	−0.0463*	−0.0065	−0.0417*	0.0415*
Numrec	−0.5101*	−0.3994*	−0.1760*	−0.0192*	−0.1070*	−0.0740*	−0.0047	−0.0719*	0.0047
Gindex	−0.1623*	−0.0855*	0.0215	0.0050	−0.0768*	−0.0597*	−0.0149	0.0128	−0.0343*

Panel B: Correlation Variables *ADJ to CFI* to *Gindex*, continued from Panel A

	Adj. to CFI	*Adj. to FCF*	*LN_Rev*	*Leverage*	*ROA*	*Tangibility*	*Audit_length*	*Numrec*	*Gindex*
Adj. to CFI	1.0000								
Adj. to FCF	0.5517*	1.0000							
LN_Rev	0.0437*	0.0439*	1.0000						
Leverage	−0.0948*	−0.0186*	−0.4254*	1.0000					
ROA	−0.0155	−0.0050	0.1359*	−0.2148*	1.0000				

(continued on next page)

TABLE 3 (continued)

	Adj. to CFI	*Adj. to FCF*	*LN_Rev*	*Leverage*	*ROA*	*Tangibility*	*Audit_length*	*Numrec*	*Gindex*
Tangibility	0.0375*	−0.0073	−0.1155*	0.0821*	0.0023	1.0000			
Audit_Length	−0.0119	0.0270*	0.2387*	−0.1723*	0.0151	−0.1126*	1.0000		
Numrec	0.0345*	0.0390*	0.5183*	−0.2685*	0.2024*	−0.0582*	0.2559*	1.0000	
Gindex	−0.0040	−0.0338*	−0.0160	−0.1337*	−0.0140	−0.0519*	0.0663*	0.0559*	1.0000

* Denotes significance at the 5 percent level.

The table reports the pairwise Pearson correlation coefficients.

Variables are defined in Appendix A.

TABLE 4

Regressions of Rating on Rating Agency Adjustments

Panel A: Regressions of Ratings on Main Rating Agency Adjustments

Model Dep. Var.	Pred. Sign	1 *Rating*	2 *Rating*	3 *Rating*	4 *Rating*	5 *Rating*	6 *Rating*	7 *Rating*	8 *Rating*
OFFBSD	(+)	2.122***			2.206***	2.348***			2.365***
		[5.294]			[7.100]	[4.129]			[4.990]
SOFT	(+)		0.632***		0.636***		0.560***		0.561***
			[25.43]		[24.86]		[14.18]		[12.64]
TOTAL	(+)			0.604***				0.515***	
				[25.94]				[12.84]	
LN_Rev	(−)	−1.263***	−1.125***	−1.164***	−1.111***	−0.704***	−0.657***	−0.691***	−0.641***
		[−23.92]	[−23.10]	[−24.40]	[−23.46]	[−9.217]	[−9.061]	[−9.435]	[−8.937]
Leverage	(+)	4.152***	4.360***	4.898***	4.264***	4.262***	4.069***	4.653***	4.259***
		[9.677]	[17.22]	[16.52]	[14.02]	[9.418]	[9.709]	[9.985]	[10.88]
ROA	(−)	−7.079***	−10.64***	−10.67***	−11.04***	−8.433***	−11.01***	−11.10***	−11.00***
		[−9.963]	[−11.26]	[−11.69]	[−12.11]	[−9.907]	[−11.98]	[−13.02]	[−12.80]
Tangibility	?	1.165**	1.739***	1.658***	1.528***	0.774*	1.552***	1.454***	1.150***
		[2.250]	[5.157]	[4.948]	[3.860]	[1.939]	[5.520]	[5.474]	[3.292]
Audit_Length	(−)					−0.0314***	−0.0168*	−0.0187**	−0.0163**
						[−3.848]	[−1.906]	[−2.326]	[−2.016]
Numrec	(−)					−0.118***	−0.105***	−0.102***	−0.103***
						[−9.047]	[−6.628]	[−7.113]	[−6.817]
Gindex	(−)					−0.0606**	−0.0369	−0.0393	−0.0378
						[−2.192]	[−1.389]	[−1.534]	[−1.604]
Constant		26.38***	25.01***	25.31***	24.79***	20.87***	20.26***	20.49***	19.91***
		[31.11]	[31.17]	[31.19]	[31.12]	[16.85]	[18.89]	[18.03]	[17.47]
Observations		1,729	1,729	1,729	1,729	817	817	817	817
R^2		0.607	0.707	0.708	0.726	0.605	0.67	0.668	0.697

(continued on next page)

TABLE 4 (continued)

Panel B: Regressions of Ratings on Alternative Measures of Rating Agency Adjustments

Model Dep. Var.	Pred. Sign	1 *Rating*	2 *Rating*	3 *Rating*	4 *Rating*	5 *Rating*	6 *Rating*	7 *Rating*	8 *Rating*	9 *Rating*	10 *Rating*
Capitalized Lease Obligation	(+)	1.822***					2.129***				
		[4.310]					[3.890]				
to Gross Profit	(+)		13.96***					25.45***			
			[3.286]					[6.151]			
Adj. to CFO	(+)			7.281*					5.542		
				[1.740]					[1.440]		
Adj. to CFI	(−)				−7.248*					−5.846**	
					[−1.871]					[−2.025]	
Adj. to FCF	?					−1.062					−1.750
						[−0.508]					[−1.027]
LN_Rev	(−)	−1.259***	−1.293***	−1.276***	−1.269***	−1.274***	−0.703***	−0.705***	−0.728***	−0.715***	−0.714***
		[−23.80]	[−24.68]	[−25.12]	[−24.04]	[−25.25]	[−9.202]	[−9.215]	[−9.727]	[−9.588]	[−9.482]
Leverage	(+)	4.146***	4.282***	4.146***	4.140***	4.248***	4.222***	4.412***	3.992***	3.949***	4.069***
		[9.917]	[10.70]	[11.12]	[10.90]	[11.72]	[9.264]	[9.730]	[8.836]	[8.691]	[8.793]
ROA	(−)	−7.149***	−6.806***	−6.836***	−6.864***	−6.708***	−8.600***	−8.084***	−8.472***	−8.469***	−8.439***
		[−9.783]	[−9.142]	[−8.999]	[−9.121]	[−9.053]	[−10.00]	[−9.523]	[−10.19]	[−10.21]	[−10.05]
Tangibility	?	1.209**	1.262***	1.377***	1.387***	1.376***	0.858**	0.927**	1.154***	1.181***	1.189***
		[2.403]	[2.750]	[3.023]	[3.029]	[3.088]	[2.229]	[2.503]	[3.080]	[3.240]	[3.123]
Audit_Length	(−)						−0.0300***	−0.0290***	−0.0318***	−0.0318***	−0.0317***
							[−3.561]	[−3.461]	[−3.631]	[−3.650]	[−3.589]
Numrec	(−)						−0.119***	−0.117***	−0.119***	−0.119***	−0.120***
							[−9.269]	[−9.362]	[−8.811]	[−9.103]	[−9.072]
Gindex	(−)						−0.0569**	−0.0608**	−0.0584**	−0.0620**	−0.0611**
							[−2.012]	[−2.144]	[−1.970]	[−2.107]	[−2.003]
Constant		26.40***	26.81***	27.86***	27.72***	27.78***	20.91***	20.72***	21.31***	21.16***	21.15***
		[31.03]	[31.94]	[33.05]	[31.81]	[33.01]	[16.90]	[16.31]	[18.04]	[17.73]	[18.13]
Observations		1,729	1,729	1,727	1,727	1,727	817	817	816	816	816
R^2		0.602	0.600	0.594	0.594	0.589	0.599	0.601	0.583	0.586	0.579

(continued on next page)

TABLE 4 (continued)

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.
The table reports the results from the OLS regression of *Rating* on *ADJ* and controls. Standard errors are clustered by firm and year. Fixed effects for year and regulated industry are included. Robust t-statistics are in brackets.
Variables are defined in Appendix A.

associated with an increase of 12.2 percent (12.3 percent) of the average rating. The estimated coefficients on *OFFBSD* and *SOFT* retain their size and significance when they are jointly included in Columns 4 and 8. These results, together with the fact that *OFFBSD* and *SOFT* have a low correlation of 0.0182, suggest that those two adjustments capture different dimensions of credit risk as they are largely orthogonal to one another. The results for the control variables are mostly in line with prior expectations. Size and profitability are associated with higher ratings, and leverage is associated with lower ratings. The additional controls, *Audit_Length, Numrec,* and *Gindex,* are available for only a subset of publicly listed firms (Columns 4–8). Longer auditor tenure, greater number of equity analysts, and a greater G-Score are associated with higher ratings.

Table 4, Panel B reports the results of estimating Equation (1) using adjustments to capitalized leases, gross profit, and cash flows. Columns 1–4 (Columns 5–8) of Panel B report the results excluding (including) *Audit_Length, Numrec,* and *Gindex.* I find that adjustments to *Capitalized Operating Leases* are significantly associated with greater *Rating*. Adjustments to *CFO,* adjustments to *CFI,* and adjustments to *Gross Profit* are positively, negatively, and positively associated with *Rating,* respectively. These results are in line with the univariate correlations, indirectly reflecting the impact of off-balance sheet debt adjustments. The control variables have the predicted signs across all specifications.[25]

In summary, the results in Table 4 indicate that rating agency adjustments for quantitative and soft factors are associated with higher ratings. The adjustments for quantitative factors primarily reflect the recognition of off-balance sheet debt. That includes adjustments to financial statements other than the balance sheet. These results provide support for the construct validity of the rating agency adjustments measures.

To test H1, that rating agency adjustments are priced in the debt market, we estimate Equation (2) using OLS and report the results in Table 5. The dependent variable in Equation (2) is *Spr5y,* the logarithm of the five-year CDS spread. Table 5, Panel A reports the estimation of models with fewer control variables, but more observations, in Columns 1–4 and with more control variables, but fewer observations, in Columns 5–8. Specifically, the model reported in Columns 5–8 includes *Audit_Length, Numrec,* and *Gindex.* Consistent with H1, the coefficients on *OFFBSD, SOFT,* and *TOTAL* are significantly positive in all eight columns. Using the estimated coefficient reported in Column 1, a one-standard-deviation increase in *OFFBSD* is associated with an increase of 16 percent in the credit spread. Using the estimated coefficient reported in Column 2 (3), a one-standard-deviation increase in *SOFT* (*TOTAL*) is associated with an increase of 39 percent (35 percent) in the credit spread. Both *OFFBSD* and *SOFT* retain their size and significance when they are jointly included in the regression in Columns 4 and 8. This implies that these two adjustments capture orthogonal dimensions of credit risk. The control variables have the predicted signs, with size and profitability (leverage) negatively (positively) associated with *Spread5y*. The additional controls for *Audit_Length, Numrec,* and *Gindex* have a negative association with *Spr5y*.

Table 5, Panel B reports the results of estimating Equation (2) using adjustments to capitalized leases, gross profit, and cash flows. Columns 1–4 (Columns 5–8) of Panel B report the results excluding (including) *Audit_Length, Numrec,* and *Gindex.* Adjustments to capitalized operating leases are significantly associated with greater *Spr5y*. Adjustments to *CFO,* adjustments to *CFI,* and adjustments to *Gross Profit* are positively, negatively, and positively associated with *Spr5y,* respectively. These results are in line with the univariate correlations, in that they indirectly reflect the impact of off-balance sheet debt adjustments. The control variables have the predicted signs across all specifications.

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[25] In untabulated robustness tests, I estimate the regressions using an ordered probit model. The results are unchanged.

TABLE 5

Regressions of Credit Spread on Rating Agency Adjustments

Panel A: Regressions of Credit Spreads on Main Rating Agency Adjustments

Model	Pred. Sign	1	2	3	4	5	6	7	8
Dep. Var.		*Spr5y*	*Spr5y*	*Spr5y*	*Spr5y*	*Spr5y*	*Spr5y*	*Spr5y*	*Spr5y*
OFFBSD	(+)	0.679***			0.687***	0.625***			0.637***
		[6.752]			[6.244]	[7.660]			[7.833]
SOFT	(+)		0.165***		0.165***		0.137***		0.137***
			[5.526]		[5.603]		[4.184]		[4.170]
TOTAL	(+)			0.143***				0.116***	
				[4.707]				[3.865]	
LN_Rev	(−)	−0.213***	−0.187***	−0.193***	−0.184***	−0.104***	−0.0948***	−0.106***	−0.0873***
		[−2.770]	[−3.102]	[−3.068]	[−3.040]	[−2.695]	[−2.707]	[−2.930]	[−2.714]
Leverage	(+)	1.853***	1.846***	2.041***	1.812***	1.845***	1.879***	2.056***	1.828***
		[9.367]	[13.67]	[12.00]	[11.58]	[5.211]	[7.675]	[9.391]	[5.502]
ROA	(−)	−3.608***	−4.240***	−4.251***	−4.339***	−3.502***	−4.098***	−4.078***	−4.160***
		[−7.560]	[−10.16]	[−9.315]	[−11.13]	[−6.220]	[−9.239]	[−8.559]	[−10.04]
Tangibility	?	0.456	0.672**	0.630**	0.611**	0.304	0.521**	0.491*	0.457*
		[1.647]	[2.503]	[2.459]	[2.139]	[1.160]	[2.024]	[1.956]	[1.694]
Audit_Length	(−)					−0.0107**	−0.007	−0.00780*	−0.006
						[−2.373]	[−1.562]	[−1.905]	[−1.641]
Numrec	(−)					−0.0280**	−0.0240**	−0.0232**	−0.0249**
						[−2.496]	[−2.306]	[−2.265]	[−2.351]
Gindex	(−)					−0.0273***	−0.0195**	−0.0213**	−0.0155*
						[−2.590]	[−1.975]	[−2.289]	[−1.664]
Constant		2.104*	1.879**	1.880**	1.820*	1.115	0.761	0.874	0.610
		[1.773]	[2.078]	[1.969]	[1.942]	[1.429]	[1.138]	[1.290]	[0.915]
Observations		477	477	477	477	403	403	403	403
R^2		0.506	0.564	0.549	0.582	0.545	0.580	0.569	0.596

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*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

(continued on next page)

TABLE 5 (continued)

Panel B: Regressions of Credit Spreads on Alternative Measures of Rating Agency Adjustments

Model Dep. Var.	Pred. Sign	1 *Spr5y*	2 *Spr5y*	3 *Spr5y*	4 *Spr5y*	5 *Spr5y*	6 *Spr5y*	7 *Spr5y*	8 *Spr5y*	9 *Spr5y*	10 *Spr5y*
Capitalized Lease Obligation	(+)	0.657***					0.598***				
		[5.998]					[8.122]				
Adj. to Gross Profit	(+)		10.70***					9.287***			
			[5.429]					[4.236]			
Adj. to CFO	(+)			2.085**					1.676		
				[2.068]					[1.545]		
Adj. to CFI	(−)				−1.357***					−1.255***	
					[−3.134]					[−4.876]	
Adj. to FCF	?					−0.213					−0.363
						[−0.241]					[−0.467]
LN_Rev	(−)	−0.214***	−0.219***	−0.220***	−0.216***	−0.216***	−0.104***	−0.108***	−0.112***	−0.109***	−0.111***
		[−2.795]	[−2.870]	[−2.846]	[−2.790]	[−2.858]	[−2.709]	[−2.878]	[−2.763]	[−2.671]	[−2.741]
Leverage	(+)	1.845***	1.894***	1.817***	1.817***	1.883***	1.827***	1.904***	1.806***	1.797***	1.886***
		[9.167]	[9.531]	[9.918]	[9.444]	[10.55]	[5.221]	[5.617]	[5.926]	[5.318]	[6.407]
ROA	(−)	−3.614***	−3.580***	−3.531***	−3.548***	−3.516***	−3.519***	−3.485***	−3.455***	−3.483***	−3.454***
		[−7.698]	[−7.345]	[−7.064]	[−7.430]	[−7.415]	[−6.337]	[−5.957]	[−5.875]	[−6.270]	[−6.266]
Tangibility	?	0.471*	0.441	0.518*	0.534**	0.520*	0.323	0.302	0.368	0.385	0.372
		[1.689]	[1.640]	[1.868]	[1.980]	[1.941]	[1.239]	[1.172]	[1.406]	[1.495]	[1.439]
Audit_Length	(−)						−0.0102**	−0.00959**	−0.0107**	−0.0107**	−0.0109**
							[−2.216]	[−2.207]	[−2.250]	[−2.252]	[−2.256]
Numrec	(−)						−0.0284**	−0.0278**	−0.0274**	−0.0275**	−0.0272**
							[−2.525]	[−2.460]	[−2.444]	[−2.458]	[−2.437]
Gindex	(−)						−0.0279***	−0.0276**	−0.0291***	−0.0308***	−0.0315***
							[−2.648]	[−2.553]	[−2.763]	[−2.964]	[−2.941]
Constant		2.136*	2.179*	2.212*	2.159*	2.157*	1.149	1.121	1.299*	1.247	1.249
		[1.819]	[1.867]	[1.895]	[1.829]	[1.882]	[1.483]	[1.477]	[1.649]	[1.563]	[1.606]
Observations		477	477	477	477	477	403	403	403	403	403
R^2		0.504	0.513	0.495	0.494	0.489	0.543	0.549	0.534	0.534	0.53

(continued on next page)

TABLE 5 (continued)

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*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.
The table reports the results from the OLS regression of *Spr5y* on *ADJ* and controls. Standard errors are clustered by firm and year. Fixed effects for year and regulated industry are included. Robust t-statistics are in brackets.
Variables are defined in Appendix A.

In summary, the results in Table 5 indicate that rating agency adjustments for quantitative and soft factors are associated with higher credit spreads. The adjustments for quantitative factors primarily reflect the recognition of off-balance sheet debt. That includes adjustments to the income statement and cash flow statement. The results imply that the rating agency's total adjustments and its qualitative assessment of credit risk arising from soft factors appear to capture true default risk given that they are priced in the public debt market.

I conduct a non-nested J-test to test whether the predicted values from one model based on rating agency adjustments add significant explanatory power to the other model based on financial ratios as reported. Table 6 reports the results of the non-nested J-test. Columns 1–2 (3–4) report the estimates for the regressions using *Rating* (*Spr5y*) as the dependent variable. The first specification (Columns 1 and 3) uses *AdjustedFirmChar* as regressors, as well as the fitted value of the model using *ReportedFirmChar*. In Column 1 of Table 6, the coefficient of $\widehat{Rating}$ is insignificant, with a t-statistic of 1.634. Because $\widehat{Rating}$ has no significant explanatory power beyond what the explanatory variables in the *ADJ* model contribute, I cannot reject that adjusted numbers explain the rating, given the alternative is to use reported numbers. The process is reversed in Column 2. In this second specification, the regressors are *ReportedFirmChar*, as well as the fitted value of the model using *AdjustedFirmChar*. In Column 2, the coefficient of the predicted value of the rating is significant, with a t-statistic of 10.65; hence, I reject the model specification based on reported numbers. According to the non-nested J-test, the rating model based on adjusted firm characteristics provides a better fit than a model based on the reported firm characteristics. This implies that rating agency adjustments to recognized GAAP numbers and qualitative factors improve the explanatory power of rating prediction models.

The same conclusion holds for the credit spread models. Compared with reported ratios, financial ratios adjusted by the rating agencies better explain default risk. In Column 3 of Table 6, the coefficient of *Spread5y* is insignificant, with a t-statistic of 1.629. Because *Spr5y* has no significant explanatory power beyond what the explanatory variables in the *ADJ* model contribute, I reject that *AdjustedFirmChar* does not explain credit spreads if the alternative is to use *ReportedFirmChar*. Column 4 reports the results from the test of a default risk model based on *ReportedFirmChar* against a model based on *AdjustedFirmChar*. Here, the coefficient of the predicted value of the *Spr5y* is significant, with a t-statistic of 6.332; hence, I reject the model specification based on *ReportedFirmChar*. According to the non-nested J-test, the default spread model based on firm characteristics that incorporate rating agency adjustments provides a better fit than a model based on the reported firm characteristics.

Overall, the evidence implies that rating agency adjustments to recognized U.S. GAAP numbers and qualitative assessments significantly improve the explanatory power of default pricing models for ratings and credit spreads. To sum up the results presented so far, ratings are more than a mechanical mapping of firm characteristics; instead, they also incorporate the rating agency's qualitative assessment of credit risk arising from soft factors. Rating agency adjustments to recognized U.S. GAAP numbers and qualitative assessments significantly improve the explanatory power of credit risk models for ratings and credit spreads.

To test H2, that rating agency adjustments decrease the steepness of the credit spread term structure, Table 7 reports the OLS estimation of Equation (5). The dependent variable in this equation is *SpreadRatio*, the proxy for the steepness of the credit spread term structure. Following the steps in Tables 4–5, I estimate models with fewer control variables, but more observations, in Columns 1–3 and with more control variables, but fewer observations, in Columns 4–6. Consistent with H2, Table 7 reports significantly positive coefficients on *SOFT* and *TOTAL*. The coefficient on *OFFBSD* is positive, but not statistically significant. The additional data requirement to calculate the slope of the term structure, *SpreadRatio*, results in a smaller sample and weaker power. Using the coefficients reported in Column 2, a one-standard-deviation increase in *SOFT* is associated with

TABLE 6

Non-Nested Model Test

Model **Dep. Var.**	**1** *Rating*	**2** *Rating*	**3** *Spr5y*	**4** *Spr5y*
Predicted *Rating*	0.225	0.970***		
	[1.634]	[10.65]		
Predicted *Spr5y*			0.378	0.905***
			[1.629]	[6.332]
t-test of predicted *Rating*	Fail to reject M0	Reject M1		
t-test of predicted *Spr5y*			Fail to reject M0	Reject M1
LN(*Total Assets*)	−0.369**	−0.061	0.007	−0.025
	[−2.303]	[−0.545]	[0.109]	[−0.464]
Leverage	3.708***	0.083	1.475***	0.226
	[4.135]	[0.168]	[2.837]	[0.599]
Coverage	−0.020	−0.001	0.003	0.001
	[−1.266]	[−0.159]	[0.539]	[0.371]
Operating Margin	3.489**	−0.274	0.344	0.123
	[2.523]	[−0.242]	[0.681]	[0.259]
ROA	−14.34***	−0.525	−3.918**	−0.880
	[−3.896]	[−0.269]	[−2.592]	[−0.964]
Tangibility	0.834*	−0.133	0.364	−0.057
	[1.963]	[−0.335]	[1.495]	[−0.256]
Audit_Length	−0.012	−0.001	−0.003	−0.001
	[−1.165]	[−0.132]	[−0.556]	[−0.172]
Numrec	−0.0912***	0.002	−0.0192**	−0.001
	[−5.963]	[0.106]	[−2.529]	[−0.123]
Gindex	−0.021	−0.005	−0.001	−0.004
	[−0.555]	[−0.140]	[−0.0360]	[−0.225]
SOFT	0.590***		0.217***	
	[6.087]		[4.283]	
TOTAL	−0.014		−0.077	
	[−0.147]		[−1.537]	
Constant	13.54***	1.360	−0.846	0.534
	[3.528]	[0.587]	[−0.814]	[0.569]
Observations	817	817	403	403
R^2	0.704	0.703	0.600	0.599

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*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.
Non-Nested J-Test:
M0: Default risk = f (Adjusted firm characteristics).
M1: Default risk = f (Reported firm characteristics).
The table reports the results from the non-nested J-test. In Columns 1 and 3, the default risk variable is regressed on the explanatory variables of M0 together with the predicted default risk, the estimated dependent variable from the regression associated with M1. If the predicted default risk has some explanatory power beyond what the explanatory variables in M0 contribute, then M0 is rejected. In Columns 2 and 4, default risk is regressed on the explanatory variables of M1 together with the predicted default risk, the estimated dependent variable from the regression associated with M0. If predicted default risk has some explanatory power beyond what the explanatory variables in M1 contribute, then M1 is rejected.

TABLE 7

Regressions of *SpreadRatio* on Rating Agency Adjustments

Model Dep. Var.	Pred. Sign	1 *SpreadRatio*	2 *SpreadRatio*	3 *SpreadRatio*	4 *SpreadRatio*	5 *SpreadRatio*	6 *SpreadRatio*
OFFBSD	(+)	0.0338			0.0048		
		[0.832]			[0.0743]		
SOFT	(+)		0.0166**			0.0150**	
			[2.513]			[1.996]	
TOTAL	(+)			0.0117**			0.00965*
				[2.152]			[1.735]
LN_Rev	(−)	−0.0265*	−0.0253**	−0.0259*	−0.0187	−0.0183	−0.0191
		[−1.824]	[−2.019]	[−1.910]	[−1.311]	[−1.422]	[−1.385]
Leverage	(+)	0.163***	0.155***	0.171***	0.141***	0.138***	0.154***
		[5.695]	[4.927]	[5.493]	[3.492]	[3.102]	[3.754]
ROA	(−)	−0.724***	−0.781***	−0.769***	−0.747***	−0.803***	−0.789***
		[−7.954]	[−10.23]	[−8.967]	[−4.812]	[−5.931]	[−5.347]
Tangibility	?	0.0693	0.0949*	0.0851	0.0464	0.0671	0.0565
		[1.445]	[1.782]	[1.631]	[0.797]	[1.021]	[0.876]
Audit_Length	(−)				0.0005	0.0009	0.0007
					[0.476]	[0.965]	[0.649]
Numrec	(−)				−0.00287*	−0.0024	−0.0025
					[−1.735]	[−1.541]	[−1.615]
Gindex	(−)				−0.0022	−0.0013	−0.0015
					[−0.934]	[−0.580]	[−0.686]
Constant		1.169***	1.165***	1.168***	0.985***	0.950***	0.967***
		[4.975]	[5.630]	[5.251]	[4.432]	[4.968]	[4.715]
Observations		366	366	366	317	317	317
R^2		0.486	0.506	0.497	0.499	0.514	0.506

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

The table reports the results from the OLS regression of *SpreadRatio* on *ADJ* and controls. Standard errors are clustered by firm and year. Fixed effects for year and regulated industry are included. Robust t-statistics are in brackets. Variables are defined in Appendix A.

an increase in *SpreadRatio* of 3.3 percent ($= 0.0166 * 2.008$), a 4.7 percent increase in the average firm's ratio of these spreads of 71.0 percent. A one-standard-deviation increase in *TOTAL* is associated with an increase in *SpreadRatio* of 2.5 percent ($= 0.0117 * 2.122$), a 3.5 percent increase in the average firm's spread ratio. The coefficients on a number of the control variables are again significant. The coefficients on *Leverage* (*LN_Rev, ROA*) are significantly positive (negative), consistent with theoretical and empirical findings that credit spread term structures are flatter for credit-riskier firms. Overall, the results reported in Table 7 are consistent with H2; rating agency adjustments are associated with flatter credit term structures. This evidence supports the theory in Duffie and Lando (2001), which suggests that uncertainty with respect to asset measurement can give rise to greater short-term credit spreads relative to long-term credit spreads.

VI. CONCLUSION

This study's results show that rating agencies make extensive adjustments to U.S. GAAP numbers reported on balance sheets, income statements, and cash flow statements. The main reason for the scope and impact of the adjustments is that credit rating agencies treat off-balance sheet finance as debt, for the most part, from the capitalization of operating leases and, to a smaller extent, from reversing the de-recognition securitizations. The rating agency's adjustments substantially increase leverage ratios. Lower reported leverage ratios likely represent a financial reporting benefit to firms engaging in off-balance sheet financing arrangements. With respect to off-balance sheet economic activity, the SEC's (2005, 22) report states the concern that "many of the areas dealing with off-balance sheet arrangements involve significant use of accounting-motivated structured transactions." To the extent that the rating agency's adjustments capture real off-balance sheet financing, leverage ratios based on reported U.S. GAAP numbers significantly understate default risk for a majority of the observations in my sample. The study provides empirical evidence that rating agencies are not fixated on bright-line recognition criteria, but incorporate many off-balance sheet financing arrangements into their ratings. In addition, ratings are a function of qualitative assessments of credit risk; these soft adjustments, on average, capture increases to credit risk. The results are consistent with rating agency adjustments improving the explanatory power of credit risk models. Rating agency adjustments are associated with flatter credit term structures, which implies that borrowers that are subject to greater rating agency adjustments are characterized by greater uncertainty that manifests in greater short-term credit spreads. The results suggest that rating agencies effectively process accounting and soft information.

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APPENDIX A

Variable Definitions

Variable	**Definition**
Firm Characteristics	
LN_Rev	Logarithm of total revenues (US$ thousands).
Leverage	Total debt divided by total assets.
ROA	Operating profit divided by total assets.
Tangibility	Tangible assets divided by total assets.
CFO	Operating cash flow.
CFI	Investing cash flow.
FCF	The sum of operating cash flow and investing cash flow (free cash flow).
Audit_Length	Length of time with same auditor (years).
Numrec	Number of equity analysts.
Gindex	G-score.
Rating Agency Adjustments	
OFFBSD	Rating agency estimate of off-balance sheet debt divided by total assets.
SOFT	Difference between the hypothetical rating based on adjusted financial ratios and the actual rating. A positive value implies a lower rating.
TOTAL	Difference between the actual rating and the hypothetical rating based on reported financial ratios. A positive value implies a lower rating.
Adj. to Gross Profit	The difference between adjusted gross profit and reported gross profit divided by total assets.
Adj. to CFO	The difference between adjusted CFO and reported CFO divided by total assets.
Adj. to CFI	The difference between adjusted CFI and reported CFI divided by total assets.
Adj.to FCF	The difference between adjusted FCF and reported FCF divided by total assets.
Credit Risk	
Rating	Moody's issuer rating.
Spread5y	Five-year-CDS spread.
Spr5y	Logarithm of five-year-CDS spread.
SpreadRatio	Ratio five-year CDS spread to 20-year CDS spread.

APPENDIX B

Illustration of Rating Process

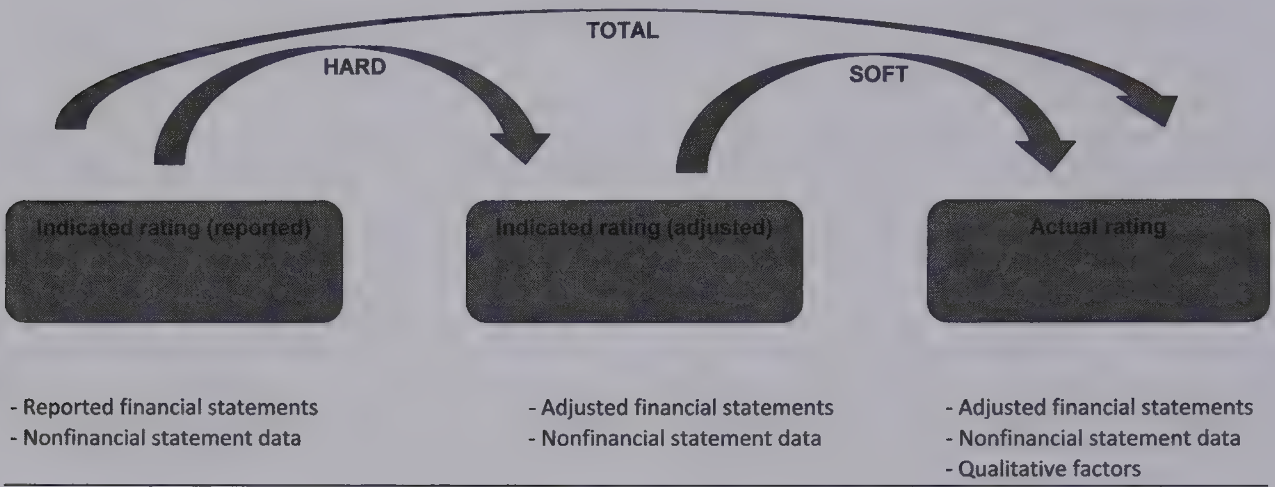
3M as of December 31, 2007	**Weight**	**As Reported**	**As Adjusted**
Factor 1: Business Profile			
Product Diversity	5.0%	Aa	Aa
Customer Diversity	5.0%	Aaa	Aaa
Regional Diversity	5.0%	Aa	Aa
Market Position	5.0%	Aaa	Aaa
End-Market Diversity	5.0%	Aaa	Aaa
Factor 2: Size and Stability			
Revenues (billions US$)	5.0%	$24.46	$24.46
Stability of Revenue Growth (STDEV)	5.0%	1.76%	1.76%
Factor 3: Cost Position and Profitability			
EBIDTA Margin (three-year avg.)	5.0%	25.03%	22.49%
ROA (EBITDA/Avg. Assets) (three-year avg.)	5.0%	26.61%	24.11%
Factor 4: Financial Policy			
Debt/Book Capital (three-year avg.)	5.0%	24.82%	37.40%
Debt/EBITDA (three-year avg.)	10.0%	0.53x	0.95x
Liquidity Assessment	10.0%	A	A
Factor 5: Financial Strength			
EBITDA/Interest Expense (three-year avg.)	10.0%	46.28x	18.78x
FFO/Debt (three-year avg.)	10.0%	120.28%	71.09%
FCF/Debt (three-year avg.)	10.0%	48.50%	30.13%
Indicated Rating (reported)		Aa1	
Indicated Rating (adjusted)			Aa2

Rating	Letter	Numeric
Indicated Rating (reported)	Aa1	2
Indicated Rating (adjusted)	Aa2	3
Actual rating	Aa1	2
HARD		1
SOFT		−1
TOTAL		0

Source: Moody's Financial Metrics.

APPENDIX C

Rating Process (Moody's Financial Metrics)


TOTAL
HARD
SOFT
Indicated rating (reported)
Indicated rating (adjusted)
Actual rating
- Reported financial statements
- Nonfinancial statement data
- Adjusted financial statements
- Nonfinancial statement data
- Adjusted financial statements
- Nonfinancial statement data
- Qualitative factors

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Product Market Power and Tax Avoidance: Market Leaders, Mimicking Strategies, and Stock Returns

Thomas R. Kubick
The University of Kansas

Daniel P. Lynch
University of Wisconsin–Madison

Michael A. Mayberry
University of Florida

Thomas C. Omer
University of Nebraska–Lincoln

ABSTRACT: Product market power provides firms with comparative advantages through more persistent profitability and insulation from competitive threats. These advantages likely provide firms with the ability to engage in greater tax avoidance. We present evidence consistent with this hypothesis. We also show that firms mimic the tax outcomes of their product market leaders. Among firms with greater product market power and comparatively high cash tax avoidance, we find stock prices to be less informative and that investors require additional compensation for the risks associated with comparatively high cash tax avoidance. Our results survive numerous robustness tests. Overall, our results suggest that industry dynamics, particularly related to a firm's competitive position, play a meaningful role in corporate tax policy.

Keywords: *tax avoidance; product market power; competition; mimicry; price-cost margin; valuation of tax avoidance.*

I. INTRODUCTION

Tax authorities continue to target high-profile market leaders in the popular press and in governmental proceedings, including Apple, GE, Google, Starbucks, and Amazon, accusing these companies of engaging in tax avoidance that costs governments billions of

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dollars in lost tax revenue.[1] A common attribute among these firms is their powerful influence in their respective product markets. In this paper, we examine how firms' product market power influences their tax avoidance strategies.

Firms do not operate in isolation, but regularly compete with one another to capture consumer market share and maximize profits. One primary dimension of competition is a firm's product market. While nearly every firm is exposed to some level of competition, the industrial organization literature has long recognized that some firms are able to influence the price, quality, and nature of the product in the marketplace to a greater extent than other product market competitors—a trait referred to as product market power (Shepherd 1970).

We conjecture that product market power offers firms some degree of insulation from competitive threats and, therefore, provides broader opportunities and potentially greater incentives for firms to engage in tax avoidance. First, product market power allows firms to maintain higher, smoother, and more persistent profitability (Hou and Robinson 2006; Irvine and Pontiff 2009; Peress 2010). Moreover, evidence suggests that firms with smooth or persistent earnings, such as those firms that possess higher degrees of product market power, are better able to forecast and realize the benefits of tax avoidance (Mayberry, McGuire, and Omer 2013).

Second, firms with greater product market power have a natural hedge against adverse outcomes (Peress 2010), allowing product market leaders to pursue riskier endeavors with less concern about negative outcomes than their competitors. For example, firms with greater product market power are able to engage in potentially risky behaviors such as holding less cash and paying more dividends (Hoberg, Phillips, and Prabhala 2014) and maintaining higher leverage (MacKay and Phillips 2005). Similar to these behaviors, tax avoidance is inherently risky (C. Chen, X. Chen, Cheng, and Shevlin 2010; Kim, Li, and Zhang 2011; Rego and Wilson 2012; Chyz, Leung, Li, and Rui 2013). Thus, product market power should reduce concerns over unsuccessful tax avoidance because the cost of unsuccessful avoidance activities is less likely to attenuate their ability to maintain their competitive position. Therefore, product market power provides a natural hedge against economic shocks, allowing for insulation from predation threats, which would support greater tax avoidance by product market leading firms.

To further examine the impact of product market power on tax avoidance, we explore how the product market leaders' tax avoidance activities influence the tax avoidance outcomes of both non-product market leading firms and its consequences to investors. First, we consider whether product market leaders' success in product markets provides incentives for other firms to mimic the tax avoidance behaviors of product market leaders in order to maintain their competitive positions. Brown (2011) and Brown and Drake (2014) document that firms mimic the tax avoidance of other firms with whom they share board interlocks. However, neither paper investigates tax-related mimicry along product market dimensions. Prior research suggests that firms mimic product market peers in other areas, such as the adoption of anti-takeover provisions and accreditation standards (Davis and Greve 1997; Chua and Petty 1999).[2] We argue that firms are likely to mimic their product market leaders on tax dimensions because the tax expense represents a material expense on a firm's financial statements (Dyreng, Hanlon, and Maydew 2008). Further, mimicking allows firms to maintain their relative performance with rivals (Chen and MacMillan 1992; Genesove and Mullin

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[1] See Norris (2013) for a discussion of Apple's senate testimony in May 2013, Kocieniewski (2011) for a popular press article on GE, and RT Network (2012) for a discussion of the British Parliament's questioning of Starbucks, Google, and Amazon.

[2] Mimicking can occur through various mechanisms. The tax avoidance of powerful firms may simply establish a social norm that firms use as a reference point. Similarly, it seems reasonable to expect that boards of directors would write contracts for tax directors that include incentives to increase tax avoidance to the product market leaders' level. Moreover, firms may attempt to mimic leading firms by making changes in tax planning or by adjusting their reporting of taxes on their financial statements. We leave the particular mechanisms open to future research.

2001) when compared on after-tax performance benchmarks. Therefore, we examine whether the tax avoidance outcomes of product market leaders influence the tax-planning activities of less powerful firms.

Second, we examine how the association between product market power and tax avoidance is valued by investors to determine the ultimate impact of these activities on firm value. We investigate the valuation implications of the tax avoidance of product market leaders because prior research suggests that firms with greater market power have lower expected returns and more informative share prices, as investors are more willing to hold and trade the shares of these firms due to decreased distress risk and less volatile cash flows and earnings (Hou and Robinson 2006; Peress 2010). The same natural hedge that results in lower expected returns and more informative stock prices is also the hypothesized mechanism leading firms with greater product market power to engage in greater tax avoidance. However, in contrast to the direct valuation implications of product market power's natural hedge, prior research considers some forms of tax avoidance to be inherently more risky (e.g., Chen et al. 2010; Kim et al. 2011; Rego and Wilson 2012). Thus, the valuation implications of product market leaders' tax avoidance are ambiguous when the activity is potentially risky and is not directly related to the less risky core product market. Thus, we investigate whether tax avoidance moderates the negative relation between product market power and subsequent stock returns, thereby reducing their informativeness (Hou and Robinson 2006; Peress 2010).[3]

Following Peress (2010), we measure product market power as the industry-adjusted, firm-specific price-cost margin (*PCM*). Firm-level price-cost margins provide an estimate of the extent to which a firm's price exceeds marginal cost. A firm with a high level of *PCM* has product market power because it does not operate in a purely competitive product market where economic profits are zero and prices are driven to marginal cost. To validate *PCM* as a reasonable measure of product market power, we begin by examining product market power and effective tax rates before and after deregulation events, which represent exogenous decreases in product market power (Gaspar and Massa 2006). Consistent with *PCM* capturing product market power, we find that *PCM* decreases following deregulation of a given industry's product market. Consistent with decreases in product market power leading to decreases in tax avoidance, we find effective tax rates also increase following these exogenous deregulation shocks. Based on these results that support *PCM* as a viable measure of product market power, we perform numerous multivariate tests to further investigate our hypotheses.

Using a sample of 25,800 firm-year observations spanning fiscal years 1993 through 2010, we find that price-cost margins are negatively related to book and cash effective tax rate (ETR) measures, suggesting that product market power is positively related to tax avoidance. Our evidence is consistent with product market power's natural hedge against negative outcomes increasing investment in tax avoidance activities. We conduct two additional tests to examine the robustness of our primary finding. First, we rank *PCM* within profit quintiles to examine the relation between *PCM* and our *ETR* measures. These rankings form distributions of *PCM* that are independent of the distribution of profitability. We continue to find a negative relation between product market power and *ETR* measures, independent of the level of profit. Second, we reestimate our model over a subsample of firms that only compete in a single product market or industry and in a single location (domestic-only firms). Restricting our analysis to this subsample allows for a more precise measure of product market power and avoids the impact of averaging the measure across multiple product markets and geographic areas. Our results and inferences remain consistent in these additional tests.

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[3] We follow Peress (2010) and assume investors are risk-averse, which makes the natural hedge property of firms with greater product market power important. Although the product market literature (e.g., Asplund 2002; Ilmakunnas 1985) generally assumes the firm is risk-neutral, we believe that our predictions would also hold under firm-level risk-aversion because the propensity to engage in inherently risky projects, such as some forms of tax avoidance, would still be influenced by the degree of product market power even if a firm is assumed to be risk-averse.

Because of their visibility and economic importance, we examine whether the actions of firms with high product market power influence other firms and investors. To investigate whether firms mimic the tax avoidance outcomes of their product market leaders, we define firms in the top tercile of *PCM* for a given industry-year as the product market leaders. We then regress firms' *TAX* on the prior-year average *TAX* of its product market leaders. We find that firms' tax avoidance is associated with the product market leaders' tax avoidance, consistent with firms attempting to mimic the tax avoidance outcomes of the product market leader. We also control for the potential that firms mimic alternative industry-wide benchmarks and/or revert to the industry mean by including the industry-average ETR. We find firms' tax avoidance is associated with the industry-average tax avoidance and that this association is significantly stronger than the association with product market leaders' tax avoidance. This result is consistent with firms using the industry average as the primary benchmark, but also with the tax avoidance of product market leaders being a relevant secondary benchmark. Because we cannot disentangle the effects of mimicking the industry average tax avoidance from mean-reversion, we interpret our results on the industry average with caution. Unlike mimicry, mean-reversion is a natural process that requires little effort. Hence, we believe our result that firms mimic the tax outcomes of product market leader firms, although economically smaller than the industry average, is nonetheless informative because, unlike the industry average, it is less susceptible to the confounding effects of mean-reversion.

In our stock returns analyses, we find a negative relation between firms' product market power and average absolute cumulative abnormal returns surrounding earnings announcement dates during the most recent fiscal year. This result is consistent with Peress' (2010) finding that, consistent with their built-in hedge against adverse outcomes making their profits less risky, firms with greater product market power have more informative stock prices because risk-averse investors are more willing to hold and trade shares of these firms. In addition, we find that the interaction between cash tax avoidance and *PCM* is positive in these regressions, suggesting that the difficulty in evaluating comparatively higher cash tax avoidance dampens the allocative efficiencies of product market power shown in prior research (Peress 2010).

We supplement our stock price informativeness tests with future stock return tests. We find a negative relation between firms' product market power and future stock returns, consistent with investors requiring less compensation to hold these shares (Hou and Robinson 2006). We observe that the negative relation between product market power and subsequent stock returns increases by becoming less negative for firms engaged in a high level of cash tax avoidance, relative to industry peers. In other words, investors are willing to accept lower returns from firms with higher product market power because their profits are generally less risky (Hou and Robinson 2006; Peress 2010), but are less willing to do so in the presence of comparatively high cash tax avoidance. Thus, our results suggest that cash tax avoidance engenders information asymmetries that can impair, although they do not appear to eliminate, the natural hedge created by product market power, resulting in less informative stock prices.

Understanding how product market power affects firms' tax avoidance is important for several reasons. First, our results provide additional information on the broader industrial organization contexts in which firms operate and how these contexts influence firm-level behavior. Second, our results suggest that tax regulators who target recognized product market leaders with enforcement actions could derive additional revenue benefits from a decrease in tax avoidance of firms that follow product market leader tax avoidance behavior. Finally, our results suggest that studies modeling firm-level tax behaviors should also consider a firm's product market as an additional determinant of the variation in observed tax avoidance behavior.

This study contributes to multiple research literatures. First, we explore cross-sectional variation in tax avoidance behavior by incorporating concepts from the industrial organization literature (Hanlon and Heitzman 2010). While existing literature has indirectly considered the impact that competitive forces have on tax avoidance through its effects on firm strategies (Higgins, Omer, and Phillips 2015) and political costs (Mills, Nutter, and Schwab 2012), we provide direct evidence on the relation between a firm's competitive position within its industry and tax avoidance. Second, we contribute to the strategic management literature by providing evidence consistent with firms mimicking the tax avoidance outcomes of the product market leaders of their industry. Third, we contribute to the finance and accounting literature on the consequences of product market competition, which has examined the effects of market power on performance (Spence 1984; Porter 1990; Nickell 1996), capital structure (Brander and Lewis 1986; Chevalier 1995), innovation (Boone 2000), managerial contracting (Karuna 2007), and voluntary disclosure (Li 2010), by examining the impact of firm-level market power on tax avoidance. Finally, we contribute to the literature on the shareholder wealth effects of tax avoidance (Hanlon and Slemrod 2009; Kim et al. 2011) by demonstrating that investors view the tax avoidance activities of firms with greater product market power as more risky and are less willing to hold shares, thereby resulting in less informative share prices and higher subsequent stock returns.

II. RELATED LITERATURE AND HYPOTHESES DEVELOPMENT

Product Market Power

Shepherd (1970) defines product market power as a firm's ability to determine the price, quality, and nature of the product in the marketplace. Greater product market power implies that firms face fewer competitive threats. Lerner (1934) developed what is characterized in the microeconomic literature as the "best-known" measure of product market power (Scherer 1970; Elzinga and Mills 2011). In this seminal work, Lerner (1934) identifies the social loss from product market power as the difference between price and marginal cost (price-cost margin). Thus, we employ a variation of Lerner's (1934) price-cost margin to examine firms' product market power relative to industry peers.

Product Market Power and Tax Avoidance Investments

We posit that product market power functions as a natural hedge to firms' non-operating decisions, a hedge that allows firms with product market power to engage in greater tax avoidance. Firms with product market power are better able to pass along negative shocks to consumers by increasing prices, whereas firms with little product market power are more appropriately characterized as "price takers." The ability to insure against negative shocks to profitability through increased product prices results in smoother and more predictable earnings compared to firms in more competitive industries (Hou and Robinson 2006; Irvine and Pontiff 2009; Peress 2010). Therefore, product market power may provide greater incentives to identify and implement tax planning.[4]

Smooth taxable income increases the precision of managers' forecasts about future taxable income, increasing the likelihood that future tax benefits will be realized and providing a better understanding of the extent to which a particular tax avoidance technology will have positive net present value (Mayberry et al. 2013). For example, firms are less likely to incur the costs of setting up an income-shifting strategy when the probability of future income is lower or the volatility of future taxable income reduces the ability to determine an estimate of the future tax savings. The low probability of realizing future income influences the current-period decision to engage in tax avoidance. Similarly, McGuire, Omer, and Wilde (2014) find that firms with higher operating uncertainty are less likely to engage in tax sheltering, apparently because resources are scarce and better returns can be derived from other investment opportunities. We expect that product market power's consistent and predictable future profits will make tax avoidance activities less costly and also offer a broader array of profitable tax avoidance technologies.

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[4] Although profitability is a previously documented determinant of tax avoidance, our argument is distinct from prior profitability studies. For example, prior literature primarily argues that profitability proxies for resources that can be invested in costly tax avoidance strategies, as well as how profitability interacts with tax preferences (Gupta and Newberry 1997; Rego 2003). While we do not dispute this with regard to product market power, we argue for a different mechanism. High levels of profitability are, on average, mean-reverting (Freeman and Tse 1992; Fairfield, Ramnath, and Yohn 2009; Ali and Zarowin 1992) and only provide assurance of resources in the current period after they have been realized. Product market power provides a higher level of certainty of future profitability, increasing the certainty that future tax benefits derived from tax avoidance strategies will occur. In addition to the increased confidence that managers have in the realization of future tax benefits, increased certainty in operations should also increase the number of tax avoidance technologies or strategies available to the firm.

Product market power's natural hedge reduces the threat that competitors' investments or other actions could erode a firm's current competitive position (Akdoğu and MacKay 2012). For firms with greater product market power, unsuccessful tax policies are less likely to impact such firms' competitive position and, thus, product market power enables greater risk-taking. For example, firms with greater product market power are able to hold less cash and pay more dividends than firms in more competitive market contexts (Hoberg et al. 2014), likely because intense competition necessitates quick responses to rivals, requiring firms with lower power to keep cash on hand to meet competitive threats. Similarly, firms with product market power have higher leverage ratios compared to firms operating in more competitive market contexts because their cash flows to service the debt are less risky (MacKay and Phillips 2005), and firms with more product market power also set more ambitious research and development (R&D) targets (Khanna and Iansiti 1997).

Tax avoidance strategies, much like dividend payouts, leverage, and ambitious innovation milestones, can potentially have negative consequences. Firms that pay dividends may end up requiring the cash for other purposes or be unable to maintain the same level of dividends due to erosion of profit margins. In a tax avoidance context, failure may involve incurring current-period costs for future-period tax benefits that are not realized because of greater uncertainty about future taxable income, the challenge by a taxing authority and the ultimate assessment of penalties and interest, or loss of reputation. Given the natural hedge provided by product market power, tax avoidance failures are also unlikely to overturn the competitive position of a firm or result in large shocks to firms' performance metrics. Thus, it is also likely to provide these firms with the opportunity to engage in more aggressive tax avoidance activities than firms with less product market power.[5]

On balance, there appears to be more consistent evidence supporting the natural hedge of product market power broadening the opportunities/incentives of firms to engage in more tax avoidance activities. Therefore, we offer the following hypothesis:

H1: Product market power is positively associated with tax avoidance.

Mimicry of Tax Avoidance

Although individual firm-level product market power can influence firms' propensity to engage in tax avoidance, we argue that firms' competitive position relative to industry peers will impact tax avoidance decisions. Specifically, the presence of a product market leader can provide incentives for rival firms to mimic the product market leader firms' performance to maintain their relative competitive positions. Previous research has found that firms mimic the tax avoidance strategies and outcomes of firms with whom they share a social tie such as an auditor or a board interlock (Brown 2011; Brown and Drake 2014). We further argue that product markets should be a context in which mimicking occurs. First, by imitating the product market leaders within their industry, rival firms can attempt to maintain or even improve relative performance (Chen and MacMillan 1992; Genesove and Mullin 2001). Second, prior research provides evidence that firms mimic the non-tax attributes of other firms in their respective industry, including: the adoption of poison pills and golden parachutes (Davis and Greve 1997), the adoption of accreditation standards (Chua and Petty 1999), and charitable donations (Galaskiewicz and Wasserman 1989).

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[5] In contrast, political cost arguments suggest that firms with greater product market power may not engage in more tax avoidance activities. Product market power endows firms with greater profitability and, therefore, may also make firms increasingly sensitive to and potentially subject to more political costs. Firms with product market power may refrain from engaging in more tax avoidance in order to minimize political costs. Prior literature using firm size as a proxy for political costs offers mixed results (Richardson and Lanis 2007). Gallemore, Maydew, and Thornock (2014) fail to find consistent evidence of reputational costs explaining lower levels of tax avoidance. In a unique setting, Mills et al. (2012) examine a sample of federal contractors and find some evidence of political sensitivity leading to decreases in tax avoidance.

Mimicking of the tax avoidance outcomes of product market leaders can occur in several ways. Firms could engage in the specific tax-planning strategies of the product market leaders. Firms could also alter their accounting choices through tax accruals that affect generally accepted accounting principles (GAAP) ETRs. However, the manner in which mimicking is accomplished is not clear and may not be uniform across different firms. *Ex ante,* we do not claim that mimicking firms duplicate the tax-planning strategies of the product market leader. Rather, we focus on tax avoidance outcome measures, which can be affected by tax-planning strategies and reporting choices. The economic incentives to mimic the product market power firms and the relative magnitude of the tax avoidance outcomes on the income statement lead to our second hypothesis:

> **H2:** The current-period tax avoidance of non-product market leading firms is positively associated with the prior-period tax avoidance of the product market leader firms.

Valuation of Tax Avoidance

Prior research suggests that product market power serves as a natural hedge against adverse outcomes. Firms with product market power are better able to pass on negative cash flow shocks to consumers, resulting in smoother earnings and cash flows. Prior research links product market power to returns by contending that risk-averse investors are more willing to hold shares of firms with greater product market power because their profits are less risky, resulting in lower expected returns and more informative stock prices (Hou and Robinson 2006; Peress 2010).

Tax avoidance can present valuation issues for investors. Goh, Lee, Lim, and Shevlin (2013) and Hutchens and Rego (2013) suggest that tax avoidance can affect investors' assessments of future cash flows, and that tax risk can increase cash flow uncertainty. Thus, our setting allows us to test the extent to which tax avoidance diminishes investors' willingness to hold shares of firms with greater product market power. By examining this issue, we contribute to both the product market and tax literatures in examining whether potentially risky tax avoidance can impact the allocative efficiencies enjoyed by firms with greater product market power.

Tax avoidance could be perceived by investors as either positive or negative. If risk-averse investors are sensitive to the profit distributions of firms with greater product market power, then they will be concerned with corporate policies (such as tax avoidance) that can negatively affect firm value (Hanlon and Slemrod 2009; Kim et al. 2011; Chen et al. 2010). Consequently, tax avoidance could moderate the relation between product market power and stock price informativeness and stock returns. On the other hand, investors may perceive the tax avoidance of firms with greater product market power as indicating effective tax planning, generating cash flows that are no more risky than the cash flows of core operating profits. These additional cash flows from tax avoidance could be perceived as a positive outcome by investors. Ultimately, we recognize that how shareholders perceive the tax avoidance of firms with product market power is an open, empirical question. Given the competing arguments, we offer the following non-directional hypotheses:

H3a: The comparatively higher tax avoidance of firms with greater product market power is not related to stock price informativeness.

H3b: The comparatively higher tax avoidance of firms with greater product market power is not related to subsequent stock returns.

III. METHODOLOGY

Sample

Our sample is comprised of 25,800 firm-year observations spanning the period 1993–2010. We begin our sample in 1993 because of Statement of Financial Accounting Standards (SFAS) No. 109's enactment in 1992 to ensure consistency in the accounting treatment of income taxes for our sample firms. We exclude firms in the financial and utilities industries due to inherent differences in regulatory and institutional structures for these firms. We also exclude firms with negative total tax expense (Compustat TXT), cash taxes paid (Compustat TXPD), or negative pretax book income before special items (Compustat PI – Compustat SPI) because these firms are in a different tax-planning position compared to firms traditionally examined in this line of research (Dyreng et al. 2008).

Measure of Market Power

Economic theory predicts that product prices equal marginal cost in purely competitive markets. The ability of a firm to set prices in excess of marginal cost is evidence of product market power and decreased competitive threats (Lerner 1934; Gaspar and Massa 2006; Peress 2010). Accordingly, we follow prior literature (Gaspar and Massa 2006; Peress 2010) and measure a firm's product market power using its excess price-cost margin (*PCM*). Specifically, *PCM* is equal to each firm's industry-adjusted price-cost margin, defined as the ratio of a firm's sales (Compustat SALE) less cost of goods sold (Compustat COGS) less selling, general, and administrative expense (Compustat XSGA) to sales (Compustat SALE). We then adjust each firm-level *PCM* by the average price-cost margin of a given industry-year (two-digit SIC), weighted by its sales (Peress 2010). We account for industry-level *PCM* for two reasons. First, by comparing a firm's price-cost margin to its industry, we control for structural industry differences unrelated to market power (Peress 2010). Second, we weight the industry-average price-cost margin by sales, which controls for the concentration of a given industry. *PCM,* therefore, is a firm-level deviation from the mean industry-year price-cost margin. *Higher* values of the price-cost margin indicate *greater* market power and, thus, *weaker* competition for a particular firm.

Prior literature (Hou and Robinson 2006; Karuna 2007; Li 2010) primarily relies on industry-level measures of product market competition such as the Herfindahl-Hirschman Index (*HHI*). While we explicitly control for industry-level competition in our research design, our variable of interest, *PCM,* is measured at the firm-level for several reasons. First, an industry-level measure would not allow us to test variation within an industry. Given that our hypotheses relate to firm-level behavior, the use of an industry-level measure would not allow us to identify which individual firms exert product market power. For example, assume an industry has four firms competing where one firm possesses 70 percent of the market share and the remaining three firms each control 10 percent of the market. The industry concentration measures would classify the industry as highly concentrated with low competition. In contrast, by controlling for industry-level effects and employing a firm-level measure of product market power, we would classify the 70 percent market share firm has having high product market power with low competition, while the 10 percent firms would be classified as having low product market power with high competition.

Second, the use of an industry-level measure might simply capture statutory tax law provisions that vary by industry. Third, recent research has questioned the use of the Compustat-based HHI measures. For example, Ali, Klasa, and Yeung (2009) find that the omission of private firms from the calculation of the HHI results in significantly different conclusions on the level of competition within an industry. However, they do find that the HHI measures constructed with private firms are positively related to *PCM*, suggesting our measure is less susceptible to the omission of private firms in our sample.

Measure of Tax Avoidance

Previous literature defines tax avoidance as any firm behavior that decreases a firm's explicit tax liability (e.g., Dyreng et al. 2008; Hanlon and Heitzman 2010). Because we wish to capture the full range of tax avoidance activities, we employ two measures of tax avoidance calculated over two separate time windows.

The book effective tax rate, *ETR*, is our first measure of tax avoidance. *ETR* captures the firm's accrual-basis tax burden during the fiscal year and is computed as the ratio of total tax expense (Compustat TXT) to pretax book income less special items (Compustat PI – Compustat SPI). However, *ETR* only reflects tax avoidance strategies that generate permanent differences and is also subject to GAAP tax accruals, such as the valuation allowance and unrecognized tax benefits. The book effective tax rate does not capture the effects of temporary book-tax differences (i.e., deferral strategies). Our second measure of tax avoidance is the firm's cash effective tax rate, *CETR*, which reflects both temporary, or tax deferral differences, and permanent differences, and is unaffected by tax accruals. We compute *CETR* as cash taxes paid during the year (Compustat TXPD) divided by pretax book income less special items (Compustat PI – Compustat SPI).

Annual effective tax rate measures can be noisy proxies for tax avoidance due to large year-over-year fluctuations (Dyreng et al. 2008). Therefore, to minimize the transitory components of the effective tax rate measures, we use two additional long-run tax avoidance measures. We compute the long-run book effective tax rate, *LRETR*, as the sum of total tax expense (Compustat TXT) from year *t*–4 to year *t*, divided by the sum of pretax book income less special items, accumulated over the same time period (Compustat PI – Compustat SPI). Similarly, we compute the long-run cash effective tax rate, *LRCETR*, as the sum of cash taxes paid (Compustat TXPD) from year *t*–4 to year *t*, divided by the sum of pretax book income less special items (Compustat PI – Compustat SPI) from the same time period. Consistent with Dyreng et al. (2008), we constrain all four effective tax rate measures to fall within the [0,1] interval to ensure a valid economic interpretation related to tax avoidance. To summarize, *lower* values of the effective tax rates (*ETR*, *CETR*, *LRETR*, and *LRCETR*) suggest *greater* tax avoidance.

The Association between Product Market Power and Tax Avoidance (H1)

In our analysis of the effects of product market power on tax avoidance, we employ an ordinary least squares (OLS) regression based on Chen et al. (2010) that adjusts standard errors for firm-level clustering and controls for year (τ_t) effects, as follows:

$$\begin{aligned} TAX_{i,t} = {} & \gamma_0 + \gamma_1 PCM_{i,t} + \gamma_2 ROA_{i,t} + \gamma_3 ACC_{i,t} + \gamma_4 SIZE_{i,t-1} + \gamma_5 FI_{i,t} + \gamma_6 EQINC_{i,t} \\ & + \gamma_7 INTAN_{i,t} + \gamma_8 PPE_{i,t} + \gamma_9 NOL_{i,t} + \gamma_{10} \Delta NOL_{i,t} + \gamma_{11} MTB_{i,t-1} + \gamma_{12} LEV_{i,t} \\ & + \gamma_{13} FCF_{i,t} + \gamma_{14} R\&D_{i,t} + \gamma_{15} HHI_{i,t} + \tau_t + \varepsilon_{i,t}. \end{aligned} \quad (1)$$

The dependent variable (*TAX*) is one of our four proxies for tax avoidance, *ETR*, *CETR*, *LRETR*, and *LRCETR*. Our variable of interest (*PCM*) is the industry-adjusted price-cost margin measured following Peress (2010). To test our first hypothesis, we examine γ_1, the coefficient on *PCM*, which represents the effect of product market power on tax avoidance. A negative coefficient on *PCM* would suggest that firms take advantage of the economic incentives provided by product market power to engage in greater tax avoidance, consistent with H1.

We include a wide array of controls previously identified as influencing firm-level tax avoidance. Gupta and Newberry (1997) find profitability is positively associated with tax avoidance, so we include return on assets, *ROA*. Frank, Lynch, and Rego (2009) find a positive association between tax avoidance and financial reporting aggressiveness, so we include performance-matched pretax discretionary accruals, *ACC*. We control for firm size, which can proxy for both firm resources and political sensitivity (Siegfried 1974; Stickney and McGee 1982; Gupta and Newberry 1997). Because foreign jurisdictions have lower statutory tax rates than the United States (Dyreng and Lindsey 2009), we control for foreign income, *FI*. This also controls for tax-planning opportunities and the effects of foreign operations on the value of tax-planning investments (De Waegenaere, Sansing, and Wielhouwer 2012). *EQINC*, equity method income in earnings, controls for book-tax differences related to income received from unconsolidated entities (Dyreng, Hanlon, and Maydew 2010). The variable *INTAN* controls for the difference in the treatment of intangibles for book and tax purposes. We control for accelerated depreciation allowed for tax purposes by including *PPE* (Stickney and McGee 1982). We also control for net operating losses by including *NOL*, which is an indicator variable equal to 1 if the firm reports a positive tax loss carryforward during the year and the change in *NOL* (ΔNOL).

To control for differences in growth opportunities, we include the market-to-book ratio, *MTB*. We also control for leverage, *LEV*, and research and development, *R&D*. We include a measure of free cash flow, *FCF*, because prior research finds that firms with greater levels of cash holdings engage in greater tax avoidance (Dhaliwal, Huang, Moser, and Pereira 2011). Finally, we include the Herfindahl-Hirschman Index (*HHI*), which is an industry-level measure of concentration and captures cross-industry differences in market competition (Hou and Robinson 2006). *HHI* is equal to the industry-level sum of the squared firm-level market share.

Mimicry of Tax Avoidance (H2)

We examine the extent to which firms mimic the tax avoidance outcomes of product market leaders by modifying Equation (1) to include three additional variables:

$$\begin{aligned} TAX_{i,t} = {} & \gamma_0 + \gamma_1 Leader_{j,t-1} + \gamma_2 Benchmark_{j,t-1} + \gamma_3 TAX_{i,t-1} + \gamma_4 ROA_{i,t} + \gamma_5 ACC_{i,t} \\ & + \gamma_6 SIZE_{i,t-1} + \gamma_7 FI_{i,t} + \gamma_8 EQINC_{i,t} + \gamma_9 INTAN_{i,t} + \gamma_{10} PPE_{i,t} + \gamma_{11} NOL_{i,t} \\ & + \gamma_{12} \Delta NOL_{i,t} + \gamma_{13} MTB_{i,t-1} + \gamma_{14} LEV_{i,t} + \gamma_{15} FCF_{i,t} + \gamma_{16} R\&D_{i,t} + \gamma_{17} HHI_{i,t} + \tau_t \\ & + \varepsilon_{i,t}. \end{aligned} \tag{2}$$

First, we identify firms in the top tercile of product market power for a given industry-year and designate these as product market leaders.[6] We then calculate the average, lagged *TAX* of the product market leaders and include it in the model (*Leader*).[7] We designate firms in the top third of the product market power as leaders, rather than the individual firm with the highest product market power, to minimize measurement error. The coefficient γ_1 estimates the extent to which firms' individual tax avoidance is associated with the previous *TAX* of the firms' product market leaders, and can be interpreted as the extent to which an individual firm appears to mimic their product market leaders. A positive coefficient on γ_1 is consistent with H2 and suggests that firms, on average, mimic the tax avoidance of their product market leaders.

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[6] In untabulated robustness tests, we select firms in the top quartile of *PCM* as product market leaders, as well as define product markets using three-digit SIC codes. Results are inferentially similar across Equations (1) and (2).

[7] We follow prior literature and employ a lagged value in order to test mimicry (Chen and MacMillan 1992; Galaskiewicz and Wasserman 1989). In order to mimic, firms must first be aware of the tax avoidance of the product market leader and then identify and implement tax avoidance strategies that allow mimicry.

Next, to control for the use of alternative benchmarks other than the tax avoidance of product market leaders, we include one of two benchmarks in Equation (2). First, we include the average, previous-year *TAX* for firms in the bottom tercile of product market power for a given industry-year (*Not Leader*). To the extent that firms mimic peers, for reasons unrelated to product market power, γ_2 should be significantly greater than 0. To the extent that firms mimic peers with greater product market power to a greater extent than firms with less product market power, γ_1 should be statistically greater than γ_2. Second, to control for mean-reversion or the possibility that firms simply mimic industry peers regardless of product market power, we include the lagged, average *TAX* for all firms in a given industry-year (*Industry*). If firms mimic peers with greater product market power to a greater extent than the industry average, then γ_1 should be statistically greater than γ_2. However, if the industry average is the more important benchmark, then we would expect γ_2 to be statistically greater than γ_1. We interpret the coefficient on *Industry* (γ_2) with caution because we cannot disentangle mimicking the industry average from the natural process of mean-reversion. Similarly, by including the firm's own lagged *TAX* for the firm (*TAX*), we control for possibility that the coefficient on *Leader* captures a firm's own individual responses to industry-wide shocks or mean-reversion at the firm level.

The Effect of Market Power and Tax Avoidance on Stock Price Informativeness (H3a) and Stock Returns (H3b)

Our third hypothesis examines the association between the tax avoidance activities of firms with greater product market power and stock price informativeness (H3a) and future stock returns (H3b). To test these relations, we estimate the following multivariate regressions:

$$\begin{aligned} CAR_{i,t} = {} & \beta_0 + \beta_1 PCM_{i,t-1} + \beta_2 TAX\_Ind_{i,t-1} + \beta_3 PCM_{i,t-1} * TAX\_Ind_{i,t-1} + \beta_4 SIZE_{i,t-1} \\ & + \beta_5 ILLIQUITDITY_{i,t-1} + \beta_6 ROA_{i,t-1} + \beta_7 MTB_{i,t-1} + \beta_8 LEV_{i,t-1} \\ & + \beta_9 TURNOVER_{i,t-1} + \mu_{ind} + \tau_t + \varepsilon_{i,t}. \end{aligned} \tag{3a}$$

$$\begin{aligned} RET_{i,t} = {} & \beta_0 + \beta_1 PCM_{i,t-1} + \beta_2 TAX\_Ind_{i,t-1} + \beta_3 PCM_{i,t-1} * TAX\_Ind_{i,t-1} + \beta_4 \Delta EARN_{i,t} \\ & + \beta_5 BETA_{i,t} + \beta_6 SIZE_{i,t} + \beta_7 BM_{i,t} + \beta_8 LEV_{i,t} + \beta_9 IDIO_{i,t} + \beta_{10} FCF_{i,t} + \mu_{ind} + \tau_t \\ & + \varepsilon_{i,t}. \end{aligned} \tag{3b}$$

In Equation (3a), we follow Peress (2010) and measure *CAR* as the average absolute five-day cumulative abnormal return surrounding earnings announcement dates during the year. Stock price informativeness is, therefore, the inverse of *CAR* (Peress 2010).[8] We augment the model to include *TAX_Ind,* which is an indicator variable equal to 1 if the firm is in the lowest two quintiles of *TAX* (the highest quintiles of tax avoidance) defined by each industry (two-digit SIC) and fiscal year. We also include *PCM* from the most recent fiscal year, and the interaction between *PCM* and *TAX_Ind* in the model. Our coefficient of interest in both models (Equations (3a) and (3b)) is β_3. A positive (negative) coefficient estimate on β_3 in Equations (3a) or (3b) would be consistent with investors negatively (positively) valuing the tax avoidance of firms with greater product market power. The remaining variables are in the same spirit of Peress (2010) and are defined in Appendix A.

In Equation (3b), *RET* is the buy-and-hold stock return over a consecutive 12-month period beginning four months after the end of each firm fiscal year. *TAX_Ind* and *PCM* are measured as previously defined. We test the hypothesized relation between tax avoidance and product market power on stock returns using lagged values of tax avoidance (*TAX_Ind*) and product market power (*PCM*). We use this construction for several reasons. First, we want to test the extent to which tax avoidance is impounded in long-window (one-year) stock returns.[9] To accomplish this objective, we must match stock return data to our tax data during the time period this information is being impounded in returns. Because all of our data are measured on an annual basis, we compound annual stock returns beginning four months after the firm's fiscal year-end to ensure that the market has received the 10-K and had an opportunity to review disclosures and compare the relevant tax information to industry benchmarks. Second, our hypotheses are based on the conjecture that firms with greater product market power have a greater propensity to accept riskier projects, which implies an effect on future valuation, rather than prior valuation. Thus, we are interested in the future valuation implications of firms with greater product market power that also engage in higher levels of tax avoidance. Hence, we use lagged values of tax avoidance (*TAX_Ind*) and market power (*PCM*) to investigate the extent to which these activities translate into greater future stock returns.

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[8] We follow Peress (2010) and measure abnormal returns as the difference between the actual firm return and an estimated return using the Fama and French (1993) three-factor model from 250 days to five days prior to each earnings announcement. The Fama and French (1993) three-factor model controls for risk in estimating *CAR*.

We control for the change in earnings ($\Delta EARN$) during the most recent fiscal year to capture the effects of recent profitability that may impact stock returns. We also control for traditional risk factors that influence stock returns, such as beta, size, book-to-market value of equity, and leverage (Fama and French 1992, 1993). We include idiosyncratic risk (*IDIO*), measured as the mean squared error from a rolling 60-month market model regression measured at the end of the fourth month following the firm's fiscal year-end. Consistent with our reasoning in Equations (1) and (2), we also include a measure of free cash flow (*FCF*) to control for differences in firm-level resources that may simultaneously impact *TAX* and subsequent stock returns.

IV. RESULTS

Descriptive Statistics and Correlations

We present descriptive statistics in Table 1, Panel A. The mean of our *TAX* variables ranges from 28.6 percent for *CETR* to 32 percent for *LRETR*, relatively close to the U.S. statutory rate of 35 percent during our sample period. We find that *PCM* averages 0.007 with a median of −0.004. These results are consistent with product markets being relatively competitive, on average, during our sample period, as both numbers are close to zero.

Table 1, Panel B contains a correlation matrix demonstrating the univariate correlations between our *TAX* variables and our variable of interest, *PCM*. Pearson correlation coefficients are above the diagonal, while Spearman correlation coefficients are below the diagonal. We find that our four *TAX* measures are positively correlated with one another, suggesting all four capture the same underlying construct. We find that *PCM* is positively related to *ETR* and *LRETR*, but negatively related to *CETR* and *LRCETR*. However, we caution readers from overreliance on these simple correlations as they do not control for other determinants of tax avoidance. These correlations suggest the need for multivariate tests that can control for potential alternative explanatory factors, which we discuss below.

Preliminary Evidence: Deregulated Industries

We provide preliminary evidence of our hypotheses, and validating evidence for our *PCM* proxy, by examining five major exogenous deregulation events. Deregulation events are related to decreases in firm-level product market power because of increases in competition.

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[9] Ideally, we would like to measure the price reactions surrounding the dates on which investors are revising expectations based on *PCM* and *TAX*. Because relevant industry benchmarks are required, and firms issue 10-Ks on different dates, it is not possible to isolate the exact dates on which investors are reacting to this information.

TABLE 1

Descriptive Statistics and Correlations

Panel A: Descriptive Statistics

Variable	n	Mean	Std. Dev.	10th Pctl	Median	90th Pctl
ETR	25,800	0.318	0.156	0.067	0.350	0.421
CETR	25,800	0.286	0.209	0.025	0.276	0.495
LRETR	25,800	0.320	0.142	0.139	0.343	0.414
LRCETR	25,800	0.294	0.164	0.089	0.296	0.444
PCM	25,800	0.007	0.091	−0.091	−0.004	0.118
ROA	25,800	0.119	0.107	0.015	0.100	0.252
ACC	25,800	−0.001	0.103	−0.107	−0.001	0.103
SIZE	25,800	5.806	1.936	3.269	5.755	8.360
FI	25,800	0.013	0.031	0.000	0.000	0.050
EQINC	25,800	0.001	0.004	0.000	0.000	0.001
INTAN	25,800	0.146	0.207	0.000	0.057	0.431
PPE	25,800	0.320	0.255	0.061	0.250	0.698
NOL	25,800	0.367	0.482	0.000	0.000	1.000
ΔNOL	25,800	−0.004	0.050	−0.023	0.000	0.013
MTB	25,800	2.821	2.565	0.887	2.076	5.382
LEV	25,800	0.187	0.203	0.000	0.138	0.450
FCF	25,800	0.050	0.105	−0.067	0.051	0.169
R&D	25,800	0.029	0.053	0.000	0.000	0.102
HHI	25,800	790.051	646.690	345.711	572.711	1493.110

Panel A presents summary statistics for the variables used in the empirical analyses. The sample is comprised of 25,800 firm-years spanning the period 1993–2010. Effective tax rates (*ETR*, *CETR*, *LRETR*, and *LRCETR*) are constrained to lie on the [0,1] interval. Firms with negative pretax book income (Compustat $\mathrm{PI}_{i,t}$), total tax expense (Compustat $\mathrm{TXT}_{i,t}$), or cash taxes paid (Compustat $\mathrm{TXPD}_{i,t}$) are omitted from the analysis. Utilities and financial firms are excluded from the sample. All continuous variables are winsorized at the 1 percent and 99 percent levels. All variables are defined in Appendix A.

Panel B: Correlations

	ETR	*CETR*	*LRETR*	*LRCETR*	*PCM*
ETR	1.000	**0.248**	**0.464**	**0.226**	**0.034**
CETR	**0.312**	1.000	**0.253**	**0.476**	**−0.125**
LRETR	**0.608**	**0.359**	1.000	**0.449**	**0.020**
LRCETR	**0.315**	**0.608**	**0.489**	1.000	**−0.148**
PCM	**0.054**	**−0.050**	**0.059**	**−0.111**	1.000

Panel B presents correlations for our dependent variables and variables of interest. Pearson correlation coefficients are above the diagonal, while Spearman correlation coefficients are below the diagonal. We bold coefficients that are significant at $p < 0.05$. The sample is comprised of 25,800 firm-years spanning the period 1993–2010. Effective tax rates (*ETR*, *CETR*, *LRETR*, and *LRCETR*) are constrained to lie on the [0,1] interval. Firms with negative pretax book income (Compustat $\mathrm{PI}_{i,t}$), total tax expense (Compustat $\mathrm{TXT}_{i,t}$), or cash taxes paid (Compustat $\mathrm{TXPD}_{i,t}$) are omitted from the analysis. Utilities and financial firms are excluded from the sample. All continuous variables are winsorized at the 1 percent and 99 percent levels. All variables are defined in Appendix A.

(continued on next page)

Examining exogenous shocks to product market power and the changes to *PCM* subsequent to those shocks should provide evidence in a context that enables us to use *PCM* as a proxy for product market power while reducing the likelihood that our results are due to correlated, omitted variables.

TABLE 1 (continued)

Panel C: Validation Tests

	(1) Pre-Deregulation	(2) Post-Deregulation	(2) − (1) Difference	t-stat	p-value
PCM	−0.007	−0.029	−0.022	2.46	0.007
ETR	0.354	0.363	0.009	−2.10	0.023

Panel C presents our validation tests using the deregulation events of Gaspar and Massa (2006). The sample is comprised of 4,247 firm-years five years before and after a given deregulation event. The deregulation events' industry and year are as follows: airline (1978), electricity (1978), natural gas (1978), telecommunications (1982), and transportation (1980). *ETR* is constrained to lie on the [0,1] interval. Firms with negative pretax book income (Compustat $PI_{i,t}$) or total tax expense (Compustat $TXT_{i,t}$) are omitted from the analysis. All continuous variables are winsorized at the 1 percent and 99 percent levels. All variables are defined in Appendix A.

We expect to find decreases in *PCM* and increases in *ETR* following a period of deregulation.[10] Following Gaspar and Massa (2006), our deregulation events occur in the airline (1978, SIC 4512), electricity (1978, SIC 4911 and 5063), natural gas (1978, SIC 4923–4925), telecommunications (1982, SIC 4183), and transportation industries (1980, SIC 4212–4215, 4011, and 4013). Although these exogenous events appear dated, they represent clearly observable industry interventions. We expect product market power to be theoretically consistent regardless of the time period. We compare *PCM* and *ETR* five years before and five years after the deregulation events and test for differences. We tabulate results in Table 1, Panel C. We find that the average *PCM* significantly decreases from –0.007 to –0.029 after the deregulation ($p = 0.007$). Moreover, we also find that *ETR* increases from 35.4 percent to 36.3 percent following deregulation and the difference is significant ($p = 0.023$). Our findings are consistent with *PCM* capturing a change in product market power and also suggest a positive relation between product market power and tax avoidance.

Multivariate Results

Table 2 presents the results from estimating Equation (1). The coefficient on *PCM* is negative and significant in all four columns ($p < 0.001$), consistent with firms with greater product market power having significantly lower effective tax rate measures (greater tax avoidance). These results support H1, which predicts that firms with greater product market power engage in greater tax avoidance.[11] Results suggest that a one-standard-deviation increase in *PCM* (0.09) reduces *ETR* (*LRETR*) by 1.0 percent (0.7 percent) and *CETR* (*LRCETR*) by 2.2 percent (2.0 percent). Multiplying the cash *ETR* measure coefficients by the average pretax income (PI) for our sample ($249.2 million for one year and $1,955.12 million for the accumulation of PI over five years), suggests a one-standard-deviation increase in product market power reduces annual (five-year total) taxes paid by between $5.4 million ($39 million), respectively.

The coefficients in Table 2 for most of the control variables are significant and in the predicted direction. We note that, unlike *PCM*, the coefficient on *ROA* is significant and positive in all four columns, suggesting that the relation between product market power and tax avoidance is distinct from the relation between profitability and tax avoidance. We fail to find a consistently significant

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[10] We can only use the book effective tax rate (*ETR*) because cash flow information is unavailable before 1986. Moreover, using *LRETR* would only give us a single observation in both the pre- and post-periods.

[11] We follow Peress (2010) and construct *PCM* by two-digit SIC code. We note similar means of *PCM* when we define *PCM* by three-digit SIC (0.008) and by Fama and French (1997) classification (0.013). Results are inferentially similar when estimating Equation (1) using these alternate constructions of *PCM*. Therefore, we proceed with the rest of the analysis using the same two-digit SIC *PCM* construction as in Peress (2010).

TABLE 2
OLS Regression of Tax Avoidance on Product Market Power

	TAX = ETR		*TAX = CETR*		*TAX = LRETR*		*TAX = LRCETR*	
Variable	**Estimate**	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**
Intercept	0.283	<0.0001	0.375	<0.0001	0.311	<0.0001	0.401	<0.0001
PCM	−0.114	<0.0001	−0.239	<0.0001	−0.078	<0.0001	−0.221	<0.0001
ROA	0.507	<0.0001	0.086	<0.0001	0.285	<0.0001	0.046	0.010
ACC	−0.021	0.180	−0.202	<0.0001	−0.051	0.001	−0.034	0.044
SIZE	0.006	<0.0001	0.002	0.028	0.005	<0.0001	0.000	0.981
FI	−0.365	<0.0001	−0.095	0.105	−0.173	0.001	0.067	0.254
EQINC	−1.302	0.000	−0.889	0.023	−0.706	0.056	−0.547	0.160
INTAN	0.032	<0.0001	0.008	0.409	0.038	<0.0001	0.001	0.898
PPE	−0.026	<0.0001	−0.098	<0.0001	−0.021	0.003	−0.094	<0.0001
NOL	−0.019	<0.0001	−0.063	<0.0001	−0.026	<0.0001	−0.049	<0.0001
ΔNOL	0.259	<0.0001	0.483	<0.0001	0.233	<0.0001	0.325	<0.0001
MTB	−0.006	<0.0001	−0.001	0.464	−0.002	0.001	−0.003	0.000
LEV	0.022	0.007	−0.057	<0.0001	−0.003	0.766	−0.031	0.001
FCF	−0.111	<0.0001	−0.277	<0.0001	−0.113	<0.0001	−0.064	0.001
R&D	−0.344	<0.0001	−0.347	<0.0001	−0.257	<0.0001	−0.326	<0.0001
HHI	0.000	0.985	0.000	0.906	0.000	0.221	0.000	0.348
F-values	52.73	<0.0001	67.58	<0.0001	27.49	<0.0001	44.43	<0.0001
Adj. R^2	0.11		0.10		0.08		0.12	
n	25,800		25,800		25,800		25,800	

This table reports results from estimating the following multivariate regression:

$$TAX_{i,t} = \gamma_0 + \gamma_1 PCM_{i,t-1} + \gamma_2 ROA_{i,t} + \gamma_3 ACC_{i,t} + \gamma_4 SIZE_{i,t-1} + \gamma_5 FI_{i,t} + \gamma_6 EQINC_{i,t} + \gamma_7 INTAN_{i,t} + \gamma_8 PPE_{i,t} + \gamma_9 NOL_{i,t} + \gamma_{10} \Delta NOL_{i,t} + \gamma_{11} MTB_{i,t-1} + \gamma_{12} LEV_{i,t} + \gamma_{13} FCF_{i,t} + \gamma_{14} R\&D_{i,t} + \gamma_{15} HHI_{i,t} + \tau_t + \varepsilon_{i,t}.$$

For brevity, year (τ_t) indicators are not reported. All other variables are defined in Appendix A. Standard errors are clustered by firm. All p-values are two-tailed.

coefficient on *HHI*, suggesting that the relation between product market power and tax avoidance occurs primarily at the firm level.[12]

Table 3 presents results for H2 firms mimicking the tax avoidance of their product market leaders. Panel A contains our results when benchmarking the mimicking of firms against *Not Leader*, while Panel B presents our results when using *Industry* as the mimicking benchmark. Overall, we find evidence that firms mimic the tax avoidance of product market leaders, as we find positive and significant coefficients on *Leader* in all but one specification, consistent with H2 ($p < 0.05$). We fail to find a significant coefficient on *Leader* in our *LRETR* specification when using

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[12] While our valuation tests in Equations (3a) and (3b) specifically control for risk, as is typical in the asset pricing literature, Equation (1) does not. Given that our natural hedge argument relies on the assumption of managers finding product market power useful in alleviating possible negative outcomes, it is important for us to determine whether our *PCM* proxy is simply capturing the propensity to undertake risky behaviors in general. In untabulated analysis, we supplement Equation (1) with either the Capital Asset Pricing Model's *Beta* parameter (calculated using a rolling five-year window and requiring at least 18 observations for each rolling window), as well as the historical operating cash flow volatility from the previous five years. We continue to find negative and significant coefficients on *PCM* (untabulated, $p < 0.05$) and generally find evidence of firms undertaking greater risk also engaging in greater tax avoidance. Therefore, our results hold even after adjusting for risk.

TABLE 3

Mimicking of Tax Avoidance

Panel A: ***Control Benchmark = Not Leader***

	TAX = ETR		*TAX = CETR*		*TAX = LRETR*		*TAX = LRCETR*	
Variable	**Estimate**	**p-value**	***CETR***	**p-value**	***LRETR***	**p-value**	***LRCETR***	**p-value**
Intercept	0.141	<0.0001	0.281	<0.0001	0.044	<0.0001	0.090	<0.0001
Leader	0.133	<0.0001	0.149	<0.0001	0.049	0.001	0.055	<0.0001
Not Leader	0.055	0.001	0.017	0.293	0.023	0.020	0.009	0.392
TAX_{t-1}	0.258	<0.0001	0.241	<0.0001	0.710	<0.0001	0.730	<0.0001
Controls?	Yes		Yes		Yes		Yes	
H: *Leader = Not Leader*	38.13	<0.0001	19.73	<0.0001	3162.48	<0.0001	4081.1	<0.0001
F-values	92.05	<0.0001	121.16	<0.0001	478.85	<0.0001	578.64	<0.0001
Adj. R^2	0.19		0.16		0.67		0.69	
n	22,753		22,753		22,072		21,710	

Panel B: ***Control Benchmark = Industry Average***

	TAX = ETR		*TAX = CETR*		*TAX = LRETR*		*TAX = LRCETR*	
Variable	**Estimate**	**p-value**	***CETR***	**p-value**	***LRETR***	**p-value**	***LRCETR***	**p-value**
Intercept	0.133	<0.001	0.253	<0.001	0.036	<0.001	0.085	<0.001
Leader	0.07	0.021	0.098	0.001	0.013	0.496	0.057	0.001
Industry	0.173	<0.001	0.138	<0.001	0.087	<0.001	0.019	0.465
TAX_{t-1}	0.255	<0.001	0.238	<0.001	0.708	<0.001	0.729	<0.001
Controls?	Yes		Yes		Yes		Yes	
H: *Leader = Industry*								
F-values	2.94	0.0867	0.41	0.5205	3.80	0.0514	0.99	0.3207
Adj. R^2	0.189		0.156		0.671		0.688	
n	22,834		22,834		22,152		21,789	

This table reports results from estimating the following multivariate regression:

$$TAX_{i,t} = \gamma_0 + \gamma_1 Leader_{i,t-1} + \gamma_2 Benchmark_{i,t-1} + \gamma_3 TAX_{i,t-1} + \gamma_4 ROA_{i,t} + \gamma_5 ACC_{i,t} + \gamma_6 SIZE_{i,t-1} + \gamma_7 FI_{i,t} + \gamma_8 EQINC_{i,t} + \gamma_9 INTAN_{i,t} + \gamma_{10} PPE_{i,t} + \gamma_{11} NOL_{i,t} + \gamma_{12} \Delta NOL_{i,t} + \gamma_{13} MTB_{i,t-1} + \gamma_{14} LEV_{i,t} + \gamma_{15} FCF_{i,t} + \gamma_{16} R\&D_{i,t} + \gamma_{17} HHI_{i,t} + \tau_t + \varepsilon_{i,t}.$$

$Leader_{i,t-1}$ represents average lagged value of *TAX* for the top tercile of product market power. For brevity, year (τ_t) indicators are not reported. Standard errors are clustered by firm. All p-values are two-tailed.

Industry as the control benchmark group. Overall, our results suggest that firms' short-run GAAP and cash ETRs are associated with the average GAAP or cash ETRs of their product market leaders in the prior period, consistent with firms mimicking the tax avoidance of their product market leaders. We also find positive and significant coefficients on *Not Leader* in the *ETR* and *LRETR* specifications, as well as on *Industry* in the *ETR*, *CETR*, and *LRETR* specifications (Panel B), consistent with firms mimicking peers for reasons other than product market power. Finally, we find that firms' tax avoidance exhibits a high level of autocorrelation. The coefficients on lagged *TAX* are positive and significant in all eight specifications ($p < 0.001$).

We interpret our results that firms mimic their product market leaders with caution, as we cannot definitively isolate how firms change their tax avoidance activities in response to the disclosed tax avoidance outcomes of product market leaders. Moreover, we fail to find consistent evidence that firms exclusively, or even primarily, mimic product market leaders. For example, in all four columns of Table 3, Panel A, F-tests reveal that the coefficients on *Leader* are significantly greater than the coefficients on *Not Leader* ($p < 0.05$). However, also using F-tests, in Panel B, we find that the coefficient on *Industry* is significantly greater than the coefficients on *Leader* in the *ETR* and *LRETR* specifications. We find an insignificant difference in coefficients in the *CETR* specification. Overall, we cannot definitively conclude whether firms mimic the tax avoidance of leaders or the industry average to a greater extent, as we cannot distinguish active mimicking of the industry average (*Industry*) versus the natural process of mean-reversion. Thus, we interpret these results with caution. However, we can conclude that firms' tax avoidance is correlated with the tax avoidance of product market leaders and it is incremental to any industry-level effects.

Table 4, Panel A presents results from estimating Equation (3a). Consistent with Peress (2010), *PCM* is negatively associated with average absolute five-day cumulative abnormal returns (*CAR*) surrounding earnings announcements in the following year. Therefore, we confirm Peress's (2010) finding that firms with greater product market power have more informative stock prices. We also observe a positive coefficient on *TAX_Ind* across all four specifications, suggesting that among firms with zero product market power, comparatively high tax avoidance is associated with less informative stock prices. The coefficient on the interaction between *PCM* and *TAX_Ind* in the *ETR* regression is negative and marginally significant ($p = 0.072$), and insignificant (but positive) in the *LRETR* regression. However, in both the *CETR* and *LRCETR* regressions, we observe a positive and highly significant ($p < 0.05$) coefficient on the interaction between *PCM* and *TAX_Ind,* suggesting that when coupled with product market power, comparatively higher cash tax avoidance is associated with less informative prices and rejecting H3a.[13] The magnitudes of the coefficients on the interaction term in the *CETR* and *LRCETR* regression are smaller than the magnitude observed on *PCM,* suggesting that comparatively higher cash tax avoidance partially, but not completely, moderates the relation between product market power and stock price informativeness (confirmed with untabulated F-tests). For example, a one-standard-deviation increase in *PCM* (0.09) implies a lower *CAR* by approximately 45 basis points in the *LRCETR* regression provided the firm does not also exhibit comparatively higher cash tax avoidance, which would imply an incremental increase of approximately 25 basis points. Tax avoidance may, therefore, engender information asymmetries that impair, but not eliminate, the allocative efficiencies of product market power documented in Peress (2010).

We acknowledge that our results could be sensitive to the choice of tax avoidance proxy. We attempt to address this issue by employing factor analysis to extract a common tax avoidance factor, *TAX_FACTOR,* among all four of our primary tax proxies (*ETR, CETR, LRETR,* and *LRCETR*) and include it in Equation (3a).[14] We find a positive and significant coefficient (Table 4, Panel A, $p < 0.001$) when using our *TAX_FACTOR* variable for *TAX_Ind,* and a positive and significant coefficient on the interaction term $PCM * TAX\_Ind$ ($p = 0.02$).

Table 4, Panel B presents results from estimating Equation (3b). Consistent with prior research (Hou and Robinson 2006), *PCM* is negatively associated with future stock returns ($p < 0.001$). We

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[13] Results are also similar when interacting *PCM* with continuous *ETR* measures instead of *TAX_Ind*. When using the continuous *ETR* measures, the interaction term in the *ETR* regression becomes insignificant.

[14] In Table 4, we use principal components factor analysis with varimax rotation to extract one common factor from our list of tax avoidance proxies (*ETR, CETR, LRETR,* and *LRCETR*). In Panel A, because the first eigenvalue is 1.40 and the remaining eigenvalues are all less than 0.135, we only retain the first eigenvalue. Initial factor loadings are 0.504 (*ETR*), 0.617 (*CETR*), 0.592 (*LRETR*), and 0.647 (*LRCETR*). After rotation, the factor pattern is 0.227 (*ETR*), 0.569 (*CETR*), 0.325 (*LRETR*), and 0.596 (*LRCETR*). The correlations between *TAX_FACTOR* and the *ETR* measures are positive, suggesting that, like the *ETR* measures, lower values of *TAX_FACTOR* reflect greater tax avoidance.

TABLE 4

Valuation of Tax Avoidance

Panel A: Stock Price Informativeness

	TAX_Ind = *ETR*		*TAX_Ind* = *CETR*		*TAX_Ind* = *LRETR*		*TAX_Ind* = *LRCETR*		*TAX_Ind* = *TAX_FACTOR*	
Variable	**Estimate**	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**
Intercept	0.182	<0.0001	0.179	<0.0001	0.181	<0.0001	0.179	<0.0001	0.179	<0.0001
PCM	−0.027	0.002	−0.044	<0.0001	−0.034	<0.0001	−0.050	<0.0001	−0.048	<0.0001
TAX_Ind	0.006	<0.0001	0.008	<0.0001	0.007	<0.0001	0.010	<0.0001	0.009	<0.0001
PCM ∗ *TAX_Ind*	−0.019	0.072	0.022	0.042	0.006	0.649	0.028	0.018	0.025	0.020
SIZE	−0.014	<0.0001	−0.014	<0.0001	−0.014	<0.0001	−0.014	<0.0001	−0.014	<0.0001
ILLIQUIDITY	0.000	0.755	0.000	0.672	0.000	0.732	0.000	0.588	0.000	0.655
ROA	−0.006	0.430	−0.006	0.413	−0.005	0.427	−0.005	0.424	−0.005	0.432
MTB	0.001	<0.0001	0.001	<0.0001	0.001	<0.0001	0.001	<0.0001	0.001	<0.0001
LEV	0.016	0.000	0.013	0.002	0.016	0.000	0.013	0.002	0.013	0.002
TURNOVER	0.000	0.250	0.000	0.351	0.000	0.316	0.000	0.407	0.000	0.363
F-values	77.10	<0.0001	78.84	<0.0001	77.48	<0.0001	78.56	<0.0001	78.60	<0.0001
Adj. R^2	0.31		0.31		0.31		0.31		0.31	
n	20,592		20,592		20,592		20,592		20,592	

Panel A reports results from estimating the following regression:

$$CAR_{i,t} = \alpha_0 + \alpha_1 PCM_{i,t-1} + \alpha_2 TAX\_Ind_{i,t-1} + \alpha_3 PCM * TAX\_Ind_{i,t-1} + \alpha_4 SIZE_{i,t-1} + \alpha_5 ILLIQUIDITY_{i,t-1} + \alpha_6 ROA_{i,t-1} + \alpha_7 MTB_{i,t-1} + \alpha_8 LEV_{i,t-1} + \alpha_9 TURNOVER_{i,t-1} + \varepsilon_{i,t}.$$

Following Peress (2010), stock price informativeness is measured as the inverse of the average absolute cumulative abnormal return (*CAR*) computed over a five-day window centered on each earnings announcement date during the year. Abnormal returns are defined as the difference between the actual return and an estimated return using the Fama and French (1993) three-factor model from 250 days to five days prior to each earnings announcement. Lower values of $CAR_{i,t}$ reflect more informative stock prices (Peress 2010). $PCM_{i,t-1}$ and $TAX\_Ind_{i,t-1}$ are defined in the same manner elsewhere in this paper. $SIZE_{i,t-1}$ is defined as the natural log of total assets. $ILLIQUIDITY_{i,t-1}$ is computed following Amihud (2002). $ROA_{i,t-1}$ is defined as pretax income divided by total assets. Market-to-book ratio, $MTB_{i,t-1}$, is computed as fiscal year-end market value of equity divided by book value of equity. Book value of equity is defined as total shareholders' equity plus deferred taxes and investment tax credit minus the book value of preferred stock. If shareholders' equity is missing, then we use common equity plus preferred stock, or assets minus liabilities, in that order. $LEV_{i,t-1}$ is defined as longer-term debt divided by total assets. $TURNOVER_{i,t-1}$ is defined as the log of the ratio of total common shares traded during the year to total shares outstanding. All variables are winsorized at the 1 percent and 99 percent levels to mitigate the influence of outliers. All p-values are two-tailed and standard errors are clustered by firm.

(continued on next page)

TABLE 4 (continued)

Panel B: The Effects of Tax Avoidance and Product Market Power on Stock Returns

	TAX = *ETR*		*TAX* = *CETR*		*TAX* = *LRETR*		*TAX* = *LRCETR*		*TAX* = *TAX_FACTOR*	
Variable	**Estimate**	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**
Intercept	−0.125	0.005	−0.140	0.002	−0.129	0.004	−0.138	0.002	−0.146	0.001
PCM	−0.499	<0.0001	−0.540	<0.0001	−0.516	<0.0001	−0.565	<0.0001	−0.553	<0.0001
TAX_Ind	−0.006	0.389	0.004	0.097	0.001	0.599	0.004	0.108	0.006	0.019
PCM ∗ *TAX_Ind*	0.027	0.719	0.128	0.094	0.075	0.327	0.180	0.017	0.165	0.032
ΔEARN	1.074	<0.0001	1.073	<0.0001	1.072	<0.0001	1.069	<0.0001	1.070	<0.0001
BETA	−0.021	0.003	−0.021	0.003	−0.021	0.003	−0.021	0.003	−0.021	0.003
SIZE	−0.015	<0.0001	−0.015	<0.0001	−0.015	<0.0001	−0.015	<0.0001	−0.014	<0.0001
BM	0.176	<0.0001	0.176	<0.0001	0.176	<0.0001	0.177	<0.0001	0.177	<0.0001
LEV	0.002	0.931	0.003	0.897	0.001	0.959	0.002	0.931	0.004	0.858
IDIO	0.891	<0.0001	0.906	<0.0001	0.889	<0.0001	0.896	<0.0001	0.911	<0.0001
FCF	0.443	<0.0001	0.443	<0.0001	0.443	<0.0001	0.442	<0.0001	0.441	<0.0001
F-values	66.84	<0.0001	67.16	<0.0001	67.03	<0.0001	67.25	<0.0001	67.30	<0.0001
Adj. R^2	0.21		0.21		0.21		0.21		0.21	
n	18,859		18,859		18,859		18,859		18,859	

Panel B reports results from estimating the following multivariate regression:

$$RET_{i,t} = \beta_0 + \beta_1 PCM_{i,t\ 1} + \beta_2 TAX\_Ind_{i,t-1} + \beta_3 TAX\_Ind_{i,t-1} * PCM_{i,t-1} + \beta_4 \Delta EARN_{i,t} + \beta_5 BETA_{i,t} + \beta_6 SIZE_{i,t} + \beta_7 BM_{i,t} + \beta_8 LEV_{i,t} + \beta_9 IDIO_{i,t} + \beta_{10} FCF_{i,t} + \mu_{ind} + \tau_t + \varepsilon_{i,t}.$$

$RET_{i,t}$ is computed as firm *i*'s buy-and-hold stock return, geometrically linked over 12 consecutive months beginning in the fourth month following the firm's fiscal year-end. Firms in the sample must have a consecutive string of returns over the previous 12-month period to have a valid observation. $TAX\_Ind_{i,t-1}$ is an indicator variable equal to 1 if the observation lies in the upper two quintiles of tax avoidance. *TAX_FACTOR* is computed by performing factor analysis and extracting a common tax factor from four tax avoidance proxies (*ETR*, *CETR*, *LRETR*, and *LRCETR*). Tax avoidance quintiles are ranked annually based upon increasing proxies for tax avoidance defined in Appendix A. Contemporaneous accounting data are matched with stock return data ending four months after the firm's fiscal year-end. Standard errors are clustered by firm. All p-values are two-tailed.

do not find consistent coefficients on *TAX_Ind*, as we find insignificant coefficients in all but the *CETR* specification, where the coefficient on *TAX_Ind* is positive and significant ($p < 0.10$). The coefficients on the interaction of tax avoidance and *PCM* are positive and significant in *CETR* and *LRCETR* specifications, rejecting H3b ($p < 0.10$). Analogous to the patterns observed in Table 3, Panel A, the magnitudes of the coefficients on the interaction term in the *CETR* and *LRCETR* regression are small relative to the magnitude observed on *PCM*, suggesting that comparatively high cash tax avoidance is a partial moderator of the relation between product market power and future stock returns. This suggests that investors require higher returns for the cash tax avoidance of firms with greater product market power.

Similar to the estimation of Equation (3a), Equation (3b) may also be sensitive to the choice of tax avoidance proxy. Thus, as in Equation (3a), we extract a common factor, *TAX_FACTOR*, among the four ETR proxies, in Equation (3b). We tabulate our results in the final column of Table 4 and find that the coefficient on the interaction between the common tax factor (*TAX_FACTOR*) and product market power (*PCM*) is positive and significant, rejecting H3b ($p < 0.05$).[15] Overall, these results are consistent with investors receiving additional compensation for the risks associated with tax avoidance strategies pursued by firms with higher levels of product market power.[16]

In both specifications (Equations (3a) and (3b)), we find that investors appear to be reacting to the cash effective tax rate and not the book effective tax rate. We attribute these differences to inherent differences in these two measures of tax avoidance. The cash effective tax rate, unlike the book effective tax rate, is unaffected by opaque income tax accruals and represents a more accurate estimate of the cash payments to tax authorities (Dhaliwal, Gleason, and Mills 2004). In contrast, the book effective tax rate is influenced by financial reporting conventions that flow through firms' tax accruals, which can make it particularly difficult for investors to evaluate firms' cash payments to tax authorities when revising their expectations for the firm.

V. ADDITIONAL ANALYSES

Product Market Power and Tax Avoidance

While we explicitly include controls for profitability (*ROA*) in Equation (1), there remains the possibility that our primary findings result from firms with additional resources investing in more tax avoidance technologies. To address this concern, we create a ranked measure of *PCM* that only captures variations in *PCM* that are independent of profitability. Using this methodology, firms with a relatively strong competitive position, but no profits, are ranked the same as firms with a relatively strong competitive position with ample profits. To accomplish this, we rank firms into quintiles of *ROA* and then, within each *ROA* quintile (*R_ROA*), we rank *PCM* into deciles. *R_PCM* is the ranked value of *PCM* within its given *ROA* quintile. Therefore, *R_PCM* varies from 0 to 9. We

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[15] In Table 4 (Panel B), we use principal components factor analysis with varimax rotation to extract one common factor from our list of tax avoidance proxies (*ETR*, *CETR*, *LRETR*, and *LRCETR*). Because the first eigenvalue is 1.72 and the remaining eigenvalues are all less than 0.28, we only retain the first eigenvalue. Initial factor loadings are 0.591 (*ETR*), 0.634 (*CETR*), 0.685 (*LRETR*), and 0.710 (*LRCETR*). After rotation, the factor pattern is 0.227 (*ETR*), 0.645 (*CETR*), 0.325 (*LRETR*), and 0.675 (*LRCETR*). In untabulated analyses, we also confirm that our results in Tables 3 and 5 are robust to using a common tax factor (extracted similarly) in lieu of the individual effective tax rates.

[16] To ensure that we are interpreting the returns results consistent with prior research (Peress 2010), we reestimate Equation (3b) using the absolute value of the firm's annualized subsequent return and find a consistent pattern. The main effect of *TAX_Ind* (regardless of the tax proxy used to construct the quintile ranking) is insignificant. The main effect of *PCM* is statistically negative, suggesting that firms with greater market power have more informative stock prices (through smaller price changes), consistent with Peress (2010). The interaction term $TAX\_Ind * PCM$ is statistically positive in all cases except for the *ETR* regression. This confirms the notion that the information asymmetries created by more aggressive tax avoidance (i.e., firms in the upper two quintiles of ranked tax avoidance) dampen the stock price informativeness of product market power, resulting in larger subsequent price changes (i.e., higher subsequent returns).

TABLE 5

OLS Regression of Tax Avoidance on Product Market Power Ranked Within Profitability Ranks

	TAX = ETR		*TAX = CETR*		*TAX = LRETR*		*TAX = LRCETR*	
Variable	**Estimate**	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**
Intercept	0.190	<0.0001	0.447	<0.0001	0.270	<0.0001	0.450	<0.0001
R_PCM	–0.004	<0.0001	–0.004	<0.0001	–0.001	0.009	–0.005	<0.0001
R_ROA	0.055	<0.0001	–0.012	<0.0001	0.027	<0.0001	–0.006	0.002
ACC	–0.004	0.786	–0.176	<0.0001	–0.035	0.018	–0.027	0.107
SIZE	0.004	<0.0001	0.001	0.310	0.004	<0.0001	0.000	0.788
FI	–0.409	<0.0001	–0.038	0.517	–0.183	0.000	0.081	0.165
EQINC	–1.252	0.000	–0.692	0.073	–0.620	0.092	–0.458	0.235
INTAN	0.027	0.001	0.012	0.212	0.035	0.0001	0.003	0.765
PPE	–0.027	<0.0001	–0.099	<0.0001	–0.022	0.002	–0.098	<0.0001
NOL	–0.018	<0.0001	–0.066	<0.0001	–0.027	<0.0001	–0.051	<0.0001
ΔNOL	0.278	<0.0001	0.468	<0.0001	0.239	<0.0001	0.321	<0.0001
MTB	–0.003	<0.0001	0.001	0.416	0.000	0.424	–0.002	0.000
LEV	0.025	0.002	–0.073	<0.0001	–0.005	0.551	–0.036	0.000
FCF	–0.062	<0.0001	–0.245	<0.0001	–0.080	<0.0001	–0.057	0.001
R&D	–0.304	<0.0001	–0.362	<0.0001	–0.241	<0.0001	–0.330	<0.0001
HHI	0.000	0.588	0.000	0.832	0.000	0.156	0.000	0.358
F-values	60.09	<0.0001	66.29	<0.0001	25.55	<0.0001	42.79	<0.0001
Adj. R^2	0.13		0.10		0.08		0.12	
n	25,800		25,800		25,800		25,800	

This table reports results from estimating the following multivariate regression:

$$\begin{aligned} TAX_{i,t} = {} & \gamma_0 + \gamma_1 R\_PCM_{i,t-1} + \gamma_2 R\_ROA_{i,t} + \gamma_3 ACC_{i,t} + \gamma_4 SIZE_{i,t-1} + \gamma_5 FI_{i,t} + \gamma_6 EQINC_{i,t} + \gamma_7 INTAN_{i,t} \\ & + \gamma_8 PPE_{i,t} + \gamma_9 NOL_{i,t} + \gamma_{10} \Delta NOL_{i,t} + \gamma_{11} MTB_{i,t-1} + \gamma_{12} LEV_{i,t} + \gamma_{13} FCF_{i,t} + \gamma_{14} R\&D_{i,t} + \gamma_{15} HHI_{i,t} \\ & + \tau_t + \varepsilon_{i,t}. \end{aligned}$$

R_PCM represents the decile rank of *PCM* within *ROA* quintiles, while *R_ROA* represents the quintile rank of *ROA*. For brevity, year (τ_t) indicators are not reported. All other variables are defined in Appendix A. Standard errors are clustered by firm. All p-values are two-tailed.

reestimate Equation (1) using these two ranked versions of *PCM* and present the results in Table 5. We find that *R_ROA* is negatively (positively) associated with the cash (GAAP) ETR proxies, suggesting that profitability increases cash tax avoidance, but decreases tax avoidance that impacts the GAAP ETR. We continue to find negative and significant coefficients on *R_PCM* in each of the four columns of Panel A ($p < 0.001$), consistent with firms' product market power, after controlling for profitability, being negatively related to tax avoidance and rejecting H1.[17]

While we provide initial evidence that our *PCM* proxy captures shifts in product market power in the previous section, we acknowledge that our measure is most theoretically congruent with a single-product, single-market firm. Therefore, we reestimate Equation (1) over a subsample of firms that only operate domestically (where pretax foreign income is zero) and as a single segment as

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[17] The coefficient estimates on the *R_ROA* variable are significantly larger than the estimates on the *R_PCM* variable in three of the four specifications (F-test, $p < 0.01$). However, examining the standardized coefficients suggests that the effect of *PCM* is between 19 and 23 percent of the effect of *ROA* on the GAAP tax avoidance measures and between 245 and 420 percent of the effect of *ROA* on the cash tax avoidance measures, suggesting significant and incremental effects to firm profitability.

identified by Compustat's Segments File. We continue to find negative and significant coefficients on *PCM* for each of our four tax avoidance proxies ($p < 0.001$), rejecting H1. Moreover, we also estimate Equation (1) over a subsample of firms that have foreign operations or multiple segments and compare the coefficients on *PCM* across the multinational or multi-segment subsample and the single-segment, domestic subsample using a Chow test. We continue to find negative and significant coefficients in multinational or multi-segment subsamples ($p < 0.05$). However, the coefficients on *PCM* for our single-segment, domestic subsample are significantly greater, using all four tax avoidance proxies ($p < 0.10$). This finding is consistent with our proxy for product market power, *PCM*, performing better in settings with relatively less measurement error.

To shed additional light on our findings, we reestimate Equation (1) with the adjustments recommended by Balakrishnan, Blouin, and Guay (2012). Specifically, we adjust each *TAX* measure for the average *TAX* for its given industry-year size quintile, thereby controlling for cross-sectional difference in tax avoidance based on industry and size. We continue to find negative and significant coefficients ($p < 0.01$) on *PCM* in all four specifications, consistent with product market power incentivizing firms to engage in a higher level of tax avoidance. Finally, we also reestimate Equation (1) after first-differencing all variables (i.e., a changes specification). Results are consistent with the primary findings, with negative and significant associations between *PCM* and all four tax avoidance proxies ($p < 0.01$).

Mimicry in Tax Avoidance

We include all of our sample firms, whether product market leaders or not, when estimating Equation (2). For product market leaders, the dependent variable is tax avoidance at time *t*, while the variable of interest is the average, lagged tax avoidance across all product market leaders at time $t-1$. To ensure that this does not produce a mechanical association, we remove product market leaders from the sample and reestimate Equation (2) using *Not Leader* as our benchmark. We continue to find positive and significant coefficients on *Leader* in all four specifications ($p < 0.05$), consistent with non-product market leader firms mimicking the tax avoidance of product market leaders.

In the previous section, we find evidence consistent with firms mimicking the tax avoidance of their product market leaders. However, *ex ante*, it appears equally likely that firms would also mimic the tax avoidance of the most profitable firms in their industry. Therefore, we provide evidence that our findings are not an artifact of firms mimicking profitable firms. We form two portfolios of mutually exclusive firms on opposite ends of the profit and product market power spectrums. One portfolio consists of firms in the top tercile of product market power, but the bottom tercile of profitability (*Only Power*), while the other portfolio consists of firms in the top tercile of profitability and the bottom tercile of product market power (*Only Profit*). By forming portfolios in this manner, there is no overlap between the two reference groups. We then calculate the average, lagged *TAX* value of each reference group and include them in Equation (2) in place of *Not Leader*, and present results in Table 6. We find positive and significant coefficients on both *Only Power* and *Only Profit* using all four specifications ($p < 0.05$). Our results are consistent with firms mimicking product market leaders, and this mimicking is distinct from the mimicking of profitable firms.

VI. CONCLUSION

Industrial economics defines product market power as firms' ability to determine the price, quality, and the nature of a product in a marketplace (Shepherd 1970). Firms with greater product market power enjoy a natural hedge because of their competitive position in the product market that provides smooth, consistent profits (Hou and Robinson 2006; Irvine and Pontiff 2009; Peress 2010) and insulation from competitive threats, allowing for additional risk-taking (Akdoğu and MacKay 2012; Hoberg et al. 2014; MacKay and Phillips 2005). We conjecture and find that this natural

TABLE 6

Mimicking of Tax Avoidance
Product Market Power versus Profitability

	TAX = ETR		*TAX = CETR*		*TAX = LRETR*		*TAX = LRCETR*	
Variable	**Estimate**	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**	**Estimate**	**p-value**
Intercept	0.139	<0.0001	0.266	<0.0001	0.041	<0.0001	0.092	<0.0001
Only Power	0.155	<0.0001	0.163	<0.0001	0.043	0.013	0.025	0.083
Only Profit	0.033	0.022	0.044	0.002	0.039	0.001	0.023	0.024
TAX_{t-1}	0.259	<0.0001	0.240	<0.0001	0.709	<0.0001	0.731	<0.0001
ROA	0.427	<0.0001	–0.015	0.453	0.200	<0.0001	0.028	0.001
ACC	0.010	0.405	–0.198	<0.0001	0.001	0.914	–0.065	<0.0001
SIZE	0.004	<0.0001	–0.003	0.001	0.003	<0.0001	0.000	0.473
FI	–0.283	<0.0001	–0.106	0.027	–0.144	<0.0001	–0.034	0.140
EQINC	–0.892	0.002	–0.450	0.150	–0.184	0.298	–0.218	0.161
INTAN	0.017	0.009	–0.004	0.605	0.005	0.172	0.000	0.925
PPE	–0.030	<0.0001	–0.086	<0.0001	–0.007	0.013	–0.028	<0.0001
NOL	–0.011	<0.0001	–0.042	<0.0001	–0.011	<0.0001	–0.020	<0.0001
ΔNOL	0.173	<0.0001	0.363	<0.0001	0.143	<0.0001	0.152	<0.0001
MTB	–0.005	<0.0001	0.000	0.475	–0.003	<0.0001	–0.001	0.005
LEV	0.022	0.002	–0.055	<0.0001	0.004	0.318	–0.024	<0.0001
FCF	–0.103	<0.0001	–0.295	<0.0001	–0.062	<0.0001	–0.104	<0.0001
R&D	–0.217	<0.0001	–0.238	<0.0001	–0.066	<0.0001	–0.103	<0.0001
HHI	0.000	0.676	0.000	0.447	0.000	0.954	0.000	0.577
F-values	91.85	<0.0001	121.83	<0.0001	483.01	<0.0001	565.76	<0.0001
Adj. R^2	0.19		0.16		0.67		0.69	
n	22,747		22,747		22,070		21,698	

This table reports results from estimating the following multivariate regression:

$$\begin{aligned} TAX_{i,t} = {} & \gamma_0 + \gamma_1 Only\ Power_{i,t-1} + \gamma_2 Only\ Profit_{i,t-1} + \gamma_3 TAX_{i,t-1} + \gamma_4 ROA_{i,t} + \gamma_5 ACC_{i,t} + \gamma_6 SIZE_{i,t-1} + \gamma_7 FI_{i,t} \\ & + \gamma_8 EQINC_{i,t} + \gamma_9 INTAN_{i,t} + \gamma_{10} PPE_{i,t} + \gamma_{11} NOL_{i,t} + \gamma_{12} \Delta NOL_{i,t} + \gamma_{13} MTB_{i,t-1} + \gamma_{14} LEV_{i,t} \\ & + \gamma_{15} FCF_{i,t} + \gamma_{16} R\&D_{i,t} + \gamma_{17} HHI_{i,t} + \tau_t + \varepsilon_{i,t}. \end{aligned}$$

$Only\ Power_{i,t-1}$ represents average lagged value of *TAX* for the top tercile of *PCM* and bottom tercile of *ROA*. $Only\ Profit_{i,t-1}$ represents average lagged value of *TAX* for the top tercile of *ROA* and bottom tercile of *PCM*. For brevity, year (τ_t) indicators are not reported. All other variables are defined in Appendix A. Standard errors are clustered by firm. All p-values are two-tailed.

hedge is positively related to tax avoidance, suggesting that product market power leads to greater incentives and/or opportunities for firms to engage in tax avoidance.

Prior literature also finds that firms with greater product market power elicit mimicry from their competitors along a wide array of behaviors in an attempt to maintain their competitive positions (Chen and MacMillan 1992; Genesove and Mullin 2001; Davis and Greve 1997; Chua and Petty 1999; Galaskiewicz and Wasserman 1989). We provide evidence that prevailing competitive forces in a product market result in competing firms moving toward the industry-average tax avoidance, suggesting that this is the primary benchmark employed. However, we also provide evidence that after controlling for the industry-average tax avoidance, firms mimic product market power leaders' tax avoidance outcomes, but to a lesser extent. This result is subject to the caveat that we are only documenting an association and cannot explicitly test or observe whether or how firms change their tax avoidance in response to disclosed product market leaders' tax avoidance outcomes. Finally, we show that investors view comparatively higher cash tax avoidance of firms with greater product market power as relatively more risky and are less willing to hold these shares than firms with comparatively lower cash tax avoidance, thereby resulting in less informative share prices and higher subsequent stock returns.

We make several contributions to the literature. First, we provide additional insight into the cross-sectional variation in tax avoidance (Hanlon and Heitzman 2010). To our knowledge, we are the first to draw on the industrial organizations literature in a tax context and to directly examine the implications of firm-level product market power on a firm's tax avoidance. Second, we contribute to the strategy literature by identifying a new dimension on which product market power leads to competitive mimicry. Third, we provide evidence on the valuation implications of product market power and its interaction with tax avoidance. Collectively, these findings should be of interest to taxing authorities who seek to maximize compliance given budget limitations and to investors who seek to balance investments in firms that are both powerful in their product markets, but also engage in higher levels of tax avoidance.

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APPENDIX A

Variable Definitions

Variable	Definition
Tax Avoidance Measures (*TAX*)	
ETR	*Book effective tax rate:* computed as total tax expense (Compustat TXT) divided by pretax book income less special items (Compustat PI − SPI).
CETR	*Cash effective tax rate:* Computed as cash taxes paid (Compustat TXPD) divided by pretax book income less special items (Compustat PI − SPI).
LRETR	*Long-run book effective tax rate:* Computed as the sum of total tax expense (Compustat TXT) over the current year and previous four years divided by the sum of pretax book income less special items (Compustat PI − SPI) over the current year and the previous four years.
LRCETR	*Long-run cash effective tax rate:* Computed as cash taxes paid (Compustat TXPD) over the current year and previous four years divided by the sum of pretax book income less special items (Compustat PI − SPI) over the current year and the previous four years.
Leader	The lagged, average *TAX* value of the top *PCM* tercile of a given industry-year.
Not Leader	The lagged, average *TAX* value of the bottom *PCM* tercile of a given industry-year.
Industry	The lagged, average *TAX* value for a given industry-year.
Profit Leader	The lagged, average *TAX* value of the top *ROA* tercile of a given industry-year.
Only Power	The lagged, average *TAX* value of the top *PCM* tercile and bottom tercile of *ROA* of a given industry-year.
Only Profit	The lagged, average *TAX* value of the top *ROA* tercile and bottom tercile of *PCM* of a given industry-year.
Product Market Power Variable	
PCM	*Price-cost margin:* computed as the excess price-cost margin for each firm every year following Peress (2010). *PCM* equals the operating profit (Compustat SALE − COGS − XSGA) divided by sales (Compustat SALE) of each firm minus the value-weighted (based on sales) industry average (based on two-digit SIC). If COGS or XSGA are missing, then we define operating profit as operating income after depreciation (Compustat OIADP).
Tax Avoidance Control Variables	
ROA	*Return on assets:* computed as pretax book income (Compustat $\mathrm{PI}_{i,t}$) divided by lagged total assets (Compustat $\mathrm{AT}_{i,t-1}$).
ACC	*Performance-matched pretax discretionary accruals:* computed following Frank et al. (2009).
SIZE	*Firm size:* computed as the natural logarithm of lagged total assets (Compustat AT) in all but Table 6. In Table 6, *SIZE* equals the natural logarithm of firm *i*'s market value of equity at the end of the fiscal year.
FI	*Foreign income:* computed as pretax income from foreign operations (Compustat PIFO) divided by lagged total assets (Compustat AT). We set missing observations of PIFO equal to 0.
EQINC	*Equity method earnings:* computed as equity in earnings (Compustat ESUB) divided by lagged total assets (Compustat AT). We set missing observations of ESUB equal to 0.
INTAN	*Intangibles:* computed as reported intangibles (Compustat INTAN) divided by lagged total assets (Compustat AT). We set missing observations of $\mathrm{INTAN}_{i,t}$ equal to 0.

(continued on next page)

APPENDIX A (continued)

Variable	Definition
PPE	*Property, plant, and equipment:* computed as net property, plant, and equipment (Compustat PPENT) divided by lagged total assets (Compustat AT). We set missing observations of PPENT equal to 0.
NOL	*Presence of NOL:* Indicator variable equal to 1 if the firm reports a positive tax loss carryforward during the year (Compustat TLCF).
ΔNOL	*Change in NOL:* computed as the change in firm i's NOL during the year scaled by lagged total assets (Compustat AT).
MTB	*Market-to-book ratio:* computed as the ratio of lagged market value of equity (Compustat PRCC_F * CSHO) to lagged book value of equity (Compustat CEQ).
LEV	*Leverage:* computed as total long-term debt (Compustat DLTT) divided by lagged total assets (Compustat AT). We set missing observations of DLTT equal to 0.
FCF	*Free cash flow:* computed as operating cash flows less capital expenditures (Compustat OANCF − CAPX) scaled by lagged total assets (Compustat AT). We set missing observations of OANCF or CAPX equal to 0.
R&D	*Research and development:* Computed as R&D expense (Compustat XRD) scaled by lagged total assets (Compustat AT). We set missing observations of XRD equal to 0.
HHI	*Herfindahl-Hirschman Index:* computed as the sum of the squared percentage of sales represented by firms in a given industry (defined by two-digit SIC classification).
Asset Pricing Variables	
CAR	*Stock price informativeness:* the inverse of the average absolute cumulative abnormal return (*CAR*) computed over a five-day window centered on each earnings announcement date during the year. Abnormal returns are defined as the difference between the actual return and an estimated return using the Fama and French (1993) three-factor model from 250 days to five days prior to each earnings announcement. Lower values of *CAR* reflect more informative stock prices (Peress 2010).
RET	*Subsequent return:* the buy-and-hold stock return over a consecutive 12-month period beginning four months after the end of each firm fiscal year.
$\Delta EARN$	*Change in Earnings:* Change in pretax earnings (Compustat PI) scaled by market value of equity (Compustat PRCC_F * Compustat CSHO).
BETA	*CAPM Beta:* computed over the previous 60-month period as of time t. Firms are required to have at least 18 observations over the prior 60-month period.
BM	*Book-to-market ratio:* computed as the natural logarithm of firm i's book-to-market value of equity ratio at the end of the fiscal year (Compustat CEQ/Compustat PRCC_F * CSHO).
IDIO	*Idiosyncratic risk:* computed as the mean squared error from a rolling 60-month market model regression ending at time t.
Turnover	*Turnover:* the log of the ratio of total common shares traded during the year to total shares outstanding.

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Do Accounting and Audit Quality Affect World Bank Lending?

Phillip T. Lamoreaux
Arizona State University

Paul N. Michas
The University of Arizona

Wendy L. Schultz
University of Manitoba

ABSTRACT: We investigate the role of accounting and audit quality in the allocation of international development aid loans provided by the World Bank. This aid is crucial to improve governance functions, infrastructure, and capital markets, and the accounting and audit environments in a country can provide the World Bank with confidence that aid is being used as intended rather than being diverted for personal or political gain. We find that development aid loans are higher for countries with stronger accounting quality, where IFRS use is mandated, and where the audit environment is stronger. However, we also find that United States geo-political interests influence these results. Specifically, the World Bank appears to "overlook" accounting and audit quality in countries where geo-political interests are relatively aligned with those of the U.S. Finally, we find that accounting and auditing matter only in countries with relatively high corruption levels, indicating that the World Bank has greater trust that accounting and auditing are of relatively high quality in low-corruption countries.

Keywords: *accounting quality; audit profession development; audit quality; corruption; financial reporting quality; geo-political interests; International Bank for Reconstruction and Development; international development aid; International Financial Reporting Standards; World Bank.*

Data Availability: *All data are publicly available.*

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I. INTRODUCTION

In this study, we investigate the role of accounting and audit quality in the allocation of international development aid loans provided by the World Bank. Access to this aid is crucial for developing countries to enable them to strengthen and modernize their infrastructures, capital markets, and governance functions. However, there has been little empirical research on whether the World Bank considers the strength of the accounting and audit environments in a country when making aid allocation decisions. The dollar amount of aid loans is large, with loans from the World Bank totaling more than $160 billion in 2010 (World Bank 2014a). This amount is larger in magnitude than the total year 2010 gross domestic product (GDP) for 143 out of 191 countries for which the World Bank provides GDP data. Further, the $160 billion represents 0.27 percent of total world GDP, 1.1 percent of combined GDP for the countries that received World Bank loans in 2010, and 1.6 percent of combined GDP for countries the World Bank classifies as "middle income" (World Bank 2014a). In summary, World Bank development aid lending is clearly substantial, both in total dollar amount and as a percentage of GDP, and is especially important for the recipient countries.

As we discuss later, the overtly stated purpose of World Bank aid is to support the economic, environmental, and social development of recipient countries. Therefore, accounting and audit infrastructures likely play a secondary role that is considered only after these more important, first-order effects. Alternatively, it is possible that accounting and auditing do not play a role at all in aid allocation decisions. We believe this latter possibility is unlikely because aid recipients are required to regularly provide audited financial information to the World Bank, and in the medium and long term, donor countries that ultimately back the World Bank are unlikely to continue to provide aid if it is diverted by corrupt politicians, bureaucrats, and dictators for their own benefit. Furthermore, the World Bank funds itself and future lending activity partially through repayments of loans. Consequently, the World Bank must rely on a reporting and auditing system that provides information about the use of loans provided to recipient countries.

Further, while the World Bank's stated purpose is to provide economic development assistance, research indicates that geo-political interests, particularly those of the United States (U.S.), influence lending activity by the bank (Meltzer 2000a).[1] The U.S. is the World Bank's largest shareholder and, thus, possesses the incentive and ability to influence lending for geo-political reasons. Therefore, this creates a situation where assessments of accounting and auditing may be secondary to not only the World Bank's stated mission and goals, but also to these geo-political interests.

Overall, we find that the World Bank lends more to countries with stronger accounting and audit quality. More specifically, loans are higher to countries where earnings are of higher quality, where there are fewer differences between International Accounting Standards (IAS) and a country's domestic accounting standards, and for countries that mandate the use of International Financial Reporting Standards (IFRS). We also find that loan amounts are higher in countries with a more developed audit profession, suggesting that the audit environment in a country also plays an important role in World Bank lending. As we describe in detail later, our measures of country-level accounting quality include the measure of earnings opacity as calculated by Bhattacharya, Daouk, and Welker (2003), a measure of earnings management similar to Leuz, Nanda, and Wysocki (2003), the number of differences between IAS and a country's domestic accounting standards as compiled by Bae, Tan, and Welker (2008), and mandated IFRS use in a country. Our measure of audit quality is taken from Michas (2011) and measures audit profession development in a country.

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[1] A stream of literature in economics and public policy explores the impact of donor interests on international aid allocation, including aid provided by the World Bank. We discuss many of these studies throughout the study.

We also find that U.S. geo-political interests influence aid allocation, because in countries that are more closely aligned with U.S. geo-political interests, accounting and audit quality do not influence loan amounts. However, in countries where these interests diverge, accounting and audit quality do play a role. Therefore, it does not appear that the World Bank considers accounting and audit quality when lending to countries that are relatively aligned with U.S. geo-political interests, which is consistent with the World Bank circumventing the evaluation of accounting and audit quality in the lending process when political interests are at play. Finally, we also find that accounting quality is associated with World Bank lending only in countries with relatively high overall corruption, suggesting that the World Bank has greater trust in the level of accounting quality in countries where there is a relatively lower risk of corruption and/or diversion in the accounting and auditing of World Bank-funded projects. We find some weaker evidence of a similar association with audit quality.

This study makes four important contributions. First, we investigate the effects of accounting and audit quality on the World Bank, a global stakeholder that has not been previously considered. Second, we investigate the role played by accounting and audit quality in the development of countries' infrastructures through development assistance provided by the World Bank. Third, we contribute to the economics and public policy literatures that investigate various determinants, including political influence, of international development aid by showing that accounting and audit quality are differentially considered based on whether a recipient country's geo-political interests are aligned with those of the U.S. (Andersen, Hansen, and Markussen 2006; Dreher, Sturm, and Vreeland 2009; Neumayer 2003c). Finally, we contribute to the growing literature that analyzes the consequences of differential accounting and audit quality across countries (Bae et al. 2008; Ball, Kothari, and Robin 2000; Barth 2008; Bhattacharya et al. 2003; Francis, Khurana, and Pereira 2003; Francis, Michas, and Seavey 2013; Leuz et al. 2003; Michas 2011).

We provide background information on World Bank lending and develop our hypotheses in Section II. In Section III, we discuss our empirical models and data and provide descriptive statistics on our variables. In Section IV, we discuss our main results and sensitivity analyses. Section V concludes the paper.

II. BACKGROUND AND HYPOTHESIS DEVELOPMENT

Background on World Bank Lending

The World Bank (the Bank) was founded in 1944 to facilitate post-World War II reconstruction and development. The Bank's current overall stated mission is to reduce global poverty and facilitate the flow of capital from more developed to less developed countries through five agencies with various goals (see World Bank [2014b] for a description of these five agencies). Further, these agencies, which together comprise what we know as the Bank, "are owned by the governments of member nations, which have the ultimate decision-making power within the organization on all matters, including policy issues" (World Bank 2014c). There are currently a total of 188 member countries that comprise the Bank. Governing decisions within the Bank are made through the Bank's Board of Governors, comprised of two appointees per country who are generally ministers of finance and/or development in a country, and the Board of Executive Directors, which is made up of the Bank's President plus 25 additional Executive Directors, all of whom are stationed full-time in Washington, DC.[2] The Board of Governors delegates the day-to-day operations of the Bank to the Executive Directors, but retains powers over Bank policies and procedures, risk management and assessment processes, the adequacy of governance and controls, and the effectiveness of development and poverty reduction activities (World Bank 2014d).

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[2] The five largest Bank shareholders are permanent members (France, Germany, Japan, the United Kingdom, and the United States), and each appoints one executive director, with the remaining 20 temporary members representing all remaining shareholder countries. The temporary members are appointed for two-year terms, while permanent members serve indefinitely.

We examine loans made to developing countries by the International Bank for Reconstruction and Development (IBRD), which is the major lending arm of the Bank. Through the IBRD, the Bank sells AAA-rated bonds to financial institutions, pension funds, and other institutional money managers and central banks around the world. The proceeds from these bond issuances, which totaled $28.8 billion in 2011, are then used to provide loans to recipient countries at interest rates far lower than these countries could secure in private international bond markets. Rates charged by the IBRD to borrower countries are between 0.27 and 1.0 percent over LIBOR (London InterBank Offered Rate) depending only on loan maturity, and rates do not differ depending on the recipient country (World Bank 2014e). The Bank underwrites these loans through member countries' government assets, and interest spread profits from issuing the loans are used to fund the Bank's global operations. The spread on loans in 2011 averaged 0.57 percent, where the interest paid by the Bank to bond investors was 0.63 percent, and the average interest earned by Bank loan issuances was 1.2 percent (World Bank 2011a).[3]

The Bank makes loans to a country for specific projects, and borrower countries must provide the Bank with documentation establishing that expenditures are made in conformity with the lending agreements, and that the country maximizes competition in the procurement of goods and services in an international bidding process.[4] After a loan commitment has been made by the Bank, disbursements are most often made through an intermediary financial institution such as a commercial bank, the country's central bank, or another institution that meets Bank requirements and standards. The intermediary then pays the contractor companies and organizations that actually provide goods, services, and construction for the project. In return, the Bank requires that annual audited financial statements, and interim unaudited statements, be provided per the schedule contained in the individual project agreement to provide support for the funding of project expenditures. If the financial statements or audit opinions reveal deficiencies in internal controls or evidence that funds have been used for ineligible expenses, then the Bank can suspend disbursements until adequate remedial action has been taken (World Bank 2006). Importantly, at the conclusion of a project, the Bank requires a final audit in accordance with "consistently applied auditing standards acceptable to the Bank" by an external auditor (World Bank 2006, 71). The auditor can be a private or governmental entity, but it must be independent. Audits are conducted in a similar fashion as audits of publicly held companies under Generally Accepted Auditing Standards (GAAS) and International Auditing Standards with tests of controls, substantive testing, and a final audit opinion being issued.[5] See Appendix B for additional general details about the IBRD.

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[3] Money loaned through the IBRD cannot be forgiven and converted into a grant. However, the Bank does experience delays and defaults in receiving loan payments, and these result in present value losses to the IBRD. As a result, the Bank's total loan loss provision is about 1.2 percent of total loans outstanding, and 0.4 percent of IBRD loans were in nonaccrual status as of June 30, 2011. When payment on any loan is overdue by 30 or more days, no new loans are issued until all overdue amounts have been received by the Bank.

[4] Some examples of projects funded by the Bank are a strengthening of Yangtze River dikes in China in 2008 that now protect about 75 million people and 1.6 million hectares of farmland from flood damage, the rebuilding of 71 schools, health clinics, and rural roads in Jamaica from 2007 to 2011 that benefited 56,000 people, and improving electricity transmission in Turkey, which increased delivery by 70 percent between 2002 and 2011 (World Bank 2012a).

[5] For example, see the "Message of Auditor General for English Version of the Audit Manual for World Bank Projects" that provides detailed requirements of project audits and auditors in China at: http://www.wgpd.org.mx/anexos/meetings/06usawbmanual_i.pdf

Hypothesis Development—Accounting Quality

In some ways, the Bank is similar to a commercial lender in that both provide loan capital to borrowers and the borrower enjoys unlimited upside potential from investments made with the capital (Jensen and Meckling 1976). In this situation, increased information about the borrower helps to limit the downside risk for the creditor and aids the creditor in pricing the debt to reflect this risk (Sengupta 1998). For Bank aid recipients, the unlimited upside potential includes such benefits as access to newly built roads that provide cheaper transportation of goods, a more efficient capital market resulting from improvements in regulatory oversight, improvements in food production that allow a country's population to be better-nourished at lower cost, leading to higher commercial productivity and rising standards of living. The Bank benefits from the interest earned to fund its operations, but faces full downside risk of the borrowed amount.

However, the lending transaction between the Bank and recipient countries also differs from commercial lending transactions because the Bank's primary lending incentives are not profit-based, as are those of traditional commercial lending transactions. First, the Bank provides financing to aid in the economic development of poor and middle-income countries that likely could not obtain financing from the international bond and lending markets at reasonable rates. Therefore, the fundamental purpose of the Bank is to provide development aid, whereas a commercial lender's primary purpose is to profit from the transaction. Second, while a commercial lender charges different interest rates based on the default risk profiles of its creditors, the Bank bases the interest rate *only* on the length of the borrowing term (World Bank 2014e). Third, there is a risk that aid provided to developing countries can be diverted by the political elite for the personal benefit of politicians, bureaucrats, and company owners that are connected to the government rather than used for the stated purpose of building the country's infrastructure. The possible benefits to this group include personal or family enrichment, control over sectors of the economy that are made possible by capital investments that are counter to the loan agreement, and, in the most extreme case, investment in a societal or even military power structure that helps government leaders, including dictators in some cases, increase control over a country's population for political benefit.[6] While the above incentives for Bank lending may outweigh any effect of accounting and audit quality, there are alternative reasons to suggest accounting and audit quality do matter.

First, in countries where accounting quality is low, the Bank is likely to incur higher monitoring costs. In these situations, the Bank must send representatives to personally oversee the implementation of project funds, which is a much costlier endeavor than relying on high-quality, independently audited financial statements to monitor a project.[7] Second, the Bank requires that financial statements be prepared in accordance with accounting standards acceptable to the Bank for funded projects (World Bank 2013, OP 10.00, 6), and since 2001, the Bank has been working with stakeholders in developing countries to assess the level of accounting and audit quality and has reported their findings in the Bank's Reports on Standards and Codes (ROSC). These reports reflect the Bank's perspective on the strength of the accounting infrastructure in a country, along with its assessment of the country's overall financial management system. Further, throughout its "Country Policy and Institutional Assessments" (CPIA), the Bank indicates it considers the quality of accounting and auditing in a country in its allocation of International Development Assistance (IDA) grants and loans (World Bank 2011b).

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[6] As evidence of such an agency conflict, more than 330 firms and individuals have been sanctioned by the Bank for engaging in fraud and corruption in Bank-financed projects since 1999. Details on this can be found at: http://web.worldbank.org/WBSITE/EXTERNAL/PROJECTS/0,,contentMDK:21254834~pagePK:41367~piPK:51533~theSitePK:40941,00.html

[7] This information was provided to us during a phone conversation with a regional World Bank Director of Operations.

The above discussion raises the question of the extent to which accounting quality will influence Bank lending. Although the fundamental purpose of the Bank is to provide development aid rather than generate returns for shareholders, we expect that higher-quality accounting information leads to lower monitoring costs for the Bank, which we expect results in more aid loans being issued to these countries, *ceteris paribus*. This leads us to our first hypothesis, stated in alternative form:

H1: The level of World Bank lending is positively associated with accounting quality in a country.

Hypothesis Development—Audit Quality

For each Bank-funded project, an independent auditor is charged with auditing the cash flows and financial statements of the project as part of the loan agreement. Consequently, in the presence of a high-quality, independent auditor, aid recipients will have less opportunity to divert aid funds for purposes other than those agreed to by the Bank. Further, the Bank requires that borrowers provide audited financial statements that are satisfactory to the Bank that report on the achievement of a project's development objectives and results (World Bank 2013, OP 10.00, 6).

The auditing infrastructure in a recipient country forms the foundation for the existence of audit firms and for individual audits of client organizations in that country. Michas (2011) provides evidence that a strong audit infrastructure is positively associated with firm-level audit quality in emerging market countries. Therefore, specific lending transactions approved by the Bank are likely influenced, *a priori,* by the level of audit quality the Bank expects will be implemented for a given project. Consequently, we expect the Bank will lend more, *ceteris paribus,* to countries with stronger audit infrastructures, leading to our second hypothesis in alternative form:

H2: The level of World Bank lending is positively associated with the strength of the auditing profession in a country.

The Influence of U.S. Geo-Political Interests

The hypotheses above investigate whether Bank personnel assess accounting and audit quality from a purely economic perspective when making loan decisions to reduce monitoring costs, and consistent with Bank policy. However, a large literature investigates political influences in the allocation of Bank funds, which appear to be contrary to the stated mission and goals in the Bank charter and presented on its website. For example, Frey and Schneider (1986) present four competing models for Bank lending: (1) the "needs" model, where aid is provided to countries in greatest need of financial assistance, (2) the "deserts" model, in which countries that "deserve" aid the most get the most, (3) the "benevolence" model, which posits that Bank officials dutifully fulfill their tasks according to the officially declared goals in the Bank's charter, and (4) the "politico-economic" model, where the Bank is regarded as a bureaucracy where individuals within the Bank further their own utility subject to economic and political factors. They find that the politico-economic model performs best and conclude that the behavior of the Bank is explained by both economic *and political* factors.

Further, in a 1998 report to the U.S. Congress, the Report of the International Financial Institution Advisory Commission recommended that the centralized structure of the Bank be altered, with regional offices around the world taking sole responsibility for aid allocation programs in order to take advantage of closer language and cultural ties and understanding (Meltzer 2000a). However, Allen Meltzer, the Commission's chairperson, believes the opposition to this restructuring was caused by a desire on the part of the U.S. Treasury to keep power and responsibility over the Bank and its programs from being shifted to other countries (Meltzer 2000b). This indicates both the influential role played by the U.S. within the Bank's organizational structure, as well as the desire by U.S. officials to keep this role intact.

Research investigates additional geo-political influences on international aid. For example, Andersen et al. (2006) provide evidence that when countries' voting in the United Nations General Assembly is more aligned with U.S. votes, these countries receive higher IDA grant aid amounts. This is consistent with Alesina and Dollar (2000), who show a similar result for bilateral aid flows from Organisation for Economic Co-operation and Development (OECD) countries. Further, Dreher et al. (2009) find that in the years when a country serves as a temporary member of the United Nations Security Council (UNSC), they receive funding for a higher number of Bank projects.[8] In a related study, Kilby (2009) shows that Bank structural adjustment loans are less dependent on macroeconomic performance in countries that are more aligned with U.S. interests, again using United Nations (U.N.) voting alignment to proxy for this confluence of interests. Finally, Neumayer (2003b) provides evidence that U.N. agencies provide more aid to countries that are closer in geographic distance to the Western world. Taken together, these examples provide evidence that international aid organizations, including the Bank, are not simply economic actors in the sense that aid is made available based on economic factors alone.

Further, this creates the possibility that the presumably non-political assessments of accounting and audit quality found in the CPIA and ROSC reports may be "overlooked" when the benefits gained by using the Bank to implement U.S. geo-political interests outweigh the cost savings gained from basing loan allocations on accounting and/or audit quality. This is a major divergence from the theoretical issues described above that apply to profit-sector lending. Finally, institutions holding Bank bonds likely do not exert pressure on the Bank to consider the ability of a recipient country to repay a Bank loan because these bonds are backed by Bank member countries' assets. In other words, bondholders would see a default only in the instance where the Bank's member countries cannot or will not repay the bond principal. As the U.S. is the Bank's major source of funding (World Bank 2010), the possibility of bond default is highly unlikely as these bonds are effectively the same, from a risk perspective, as U.S. Treasury bills. This creates room for the Bank to consider and act on geo-political interests.

Not only are the Bank's headquarters located in Washington, DC, the U.S. is also by far the largest provider of funds to the Bank, and the U.S. has the largest voting interest in the Bank.[9] Further, Kapur (2002, 63–66) discusses various reasons for the lack of countervailing pressures to U.S. interests from other Bank shareholders, including diverging interests among smaller shareholders and a lack of willingness for larger shareholders to jeopardize their wider bilateral relationship with the U.S. Therefore, the U.S. possesses both the incentives and ability to allocate aid, through the Bank, to countries that are more aligned with U.S. geo-political interests. Further, the U.S. possesses the power within the Bank's organizational structure to implement political considerations through the Bank because the president of the Bank has always been a United States citizen.[10] Given prior findings that U.S. political interests influence the Bank's lending process, these political interests may be manifest through an analysis, or lack of analysis, of the accounting and auditing environments in a country. For example, when a recipient country's interests are less aligned with the U.S., the Bank may scrutinize accounting and audit quality to reduce the Bank's costs of lending, as we discuss above. However, another important and possibly first-order influence, given the evidence discussed, is alignment with U.S. interests. Therefore, for beneficiary countries where U.S. political interests do play a strong role, the alignment of these interests may very well outweigh any consideration of accounting and audit quality in the Bank's determination of loan amounts. The Bank may overlook accounting and audit quality to some extent because the geo-political interests are so aligned and are considered more important than any cost savings that may be attained by more heavily scrutinizing a country's accounting and auditing. This leads to our third hypothesis in alternative form:

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[8] Also see Kuziemko and Werker (2006) for a very detailed description of the United Nations Security Council nomination and election process, as well as the extent to which U.S. interests play an important role in the voting decisions of temporary member countries.

[9] As of December 31, 2013, the U.S. possesses a 15.3 percent voting interest in the Bank. The next highest is the United Kingdom with 4.1 percent. See: http://siteresources.worldbank.org/BODINT/Resources/278027-1215524804501/IBRDCountryVotingTable.pdf

[10] This has historically been widely attributed to an informal agreement between the U.S. and European Bank members that an American always leads the Bank and a European always leads the International Monetary Fund. Further, see Kapur, Lewis, and Webb (1997) and Rich (1995) for additional information on U.S. influence on the World Bank.

> **H3:** The positive association between World Bank lending and accounting/audit quality is weaker in countries where geo-political interests are more highly aligned with U.S. interests.

Corruption, Accounting Quality, and Audit Quality

The relationship between corruption in a country and international development aid to that country is unclear, according to Alesina and Weder (2002). One argument made by Alesina and Weder (2002) is that corruption would lead to less international aid due to the danger that such aid may be diverted for personal gain by government officials in a recipient country. On the other hand, given that the purpose of international aid is often to improve foundational infrastructures in countries, which may be correlated with a country's corruption level, it is possible that more aid would go to more corrupt countries to alleviate these negative characteristics. Ultimately, Alesina and Weder (2002) are unable to definitively answer this important question.

However, the consideration of accounting and audit quality can provide important insights into the relationship between corruption and Bank loans. In countries where corruption is low, there is less incentive for Bank staff to analyze the quality of accounting and auditing, as the likelihood of government officials using Bank loans for something other than the intended purpose is lower. In countries with higher levels of corruption, however, Bank staff possess stronger incentives to ascertain whether the quality of accounting and auditing infrastructures is sufficient to deter improper use of Bank funds. This leads to our fourth and final hypothesis in alternative form:

> **H4:** The positive association between World Bank lending and accounting/audit quality is stronger in countries with relatively high levels of corruption compared to those with relatively low levels of corruption.

III. EMPIRICAL MODEL, SAMPLE, AND DESCRIPTIVE STATISTICS

Empirical Model

To measure the extent to which World Bank lending is influenced by both accounting (H1) and audit quality (H2) at the country level, as well as U.S. geo-political interests (H3) and overall corruption levels in a country (H4), we examine the following model:

$$\begin{aligned} \textit{World Bank Loans} = f(&\textit{Accounting Quality}, \textit{Audit Quality}, \textit{Alignment with U.S. Interests}, \\ &\textit{Corruption Levels}, \textit{Controls}, \textit{Country and Year Fixed Effects}), \end{aligned} \tag{1}$$

where *Accounting Quality* is one of four measures of accounting quality in a country, and *Audit Quality* is a measure of the development of the audit profession in a country. Given our relatively small sample size, we use Generalized Least Squares (GLS), which corrects for heteroscedasticity and autocorrelation across time and within countries to estimate our regression models (Gujarati 2003).

Our measure of loan aid is the natural log of the total amount of loans committed to a country in a year by the Bank's IBRD.[11] Our first measure of accounting quality used to test H1, earnings opacity (*OPACITY*), is calculated following Bhattacharya et al. (2003) and is a composite of three measures of accounting quality: (1) earnings aggressiveness, (2) loss avoidance, and (3) earnings smoothing. We calculate the firm-level median value of each measure within country-year groupings, and then rank each measure in deciles. We then average the three decile ranks for each country-year to form the final measure of *OPACITY*, which can range between 1 to 10 for each country-year observation, as detailed in Appendix A. We require a minimum of 20 firm observations in each country-year grouping, and all tabled results are very similar if we use minimums of either ten or 50 observations. We use a "changes" specification in all our analyses using *OPACITY*, where we calculate the change as the *OPACITY* value in year *t* less the value in year *t*–1. We also do this for all control variables in these changes models.

Our second measure of accounting quality used to test H1, earnings management (*EARN_MGMT*), follows Leuz et al. (2003) and is a composite of four measures of accounting quality: (1) smoothing of reported operating earnings using accruals, (2) smoothing and the correlation between changes in accruals and operating cash flows, (3) the magnitude of accruals, and (4) small loss avoidance. We average the four decile ranks for each country-year to form the final measure, *EARN_MGMT*, which can range between 1 to 10 for each country-year observation, as detailed in Appendix A. We again require a minimum of 20 firm observations in each country for each year. Leuz et al. (2003) calculate this variable for each country, whereas we calculate it for each country *in each year* to be able to implement a changes analysis as we do for our *OPACITY* measure. It is important to note that *OPACITY* and *EARN_MGMT* are correlated, but the Pearson (Spearman) values are 0.68 (0.62) ($p < 0.001$ for both). Therefore, while these measures are somewhat similar conceptually, they are statistically different from each other, which allows them to complement one another in our analyses.

Our third measure of accounting quality used to test H1, *IFRS_DIFF*, is the number of differences between International Accounting Standards (IAS) and a country's domestic accounting standards, based on a survey of partners in large accountancy organizations from various countries benchmarking local GAAP rules to IAS rules in place as of December 31, 2001, as compiled by Bae et al. (2008).[12] A higher score indicates greater differences between local accounting standards and IFRS, i.e., lower accounting quality. In contrast with our *OPACITY* and *EARN_MGMT* measures, *IFRS_DIFF* provides a measure of the quality of accounting *standards* in a country rather than a measure of the quality of *implemented* accounting practices. We exclude country fixed effects in this analysis, as our use of the Bae et al. (2008) categorization to determine *IFRS_DIFF* does not vary over time within or across countries and, accordingly, we are not able to implement a changes analysis here.

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[11] White and McGillivray (1995) suggest that commitments are the more relevant variable, compared to cash disbursements, to analyze the determinants of aid allocation, as commitments are "the decision variable of the donor" over which they exert full control.

[12] We label this variable *IFRS_DIFF* instead of *IAS_DIFF* for consistency in wording throughout our study, while noting that it was actually International Accounting Standards (IAS) that were used in 2001. IAS were replaced by International Financial Reporting Standards (IFRS) by the International Accounting Standards Board beginning in 2002.

Our fourth and final measure of accounting quality used to test H1 is the mandated use of International Financial Reporting Standards. The Bank is closely involved with the International Accounting Standards Board (IASB) and the International Monetary Fund (IMF) in the adoption of, and education about, IFRS around the world (IASB 2012). The Bank encourages IFRS adoption because the standards promote private sector growth, are a building block of a market-based monitoring of companies and government business enterprises, and improve the assessment and collection of taxes (Hegarty, Gielen, and Hirata Barros 2004, 1–2). Therefore, the Bank encourages IFRS adoption not as a condition to providing aid loans, but because it believes IFRS adoption contributes to an overall improvement in a country's financial reporting environment. Given this, the Bank clearly considers IFRS to be a benchmark in terms of accounting quality. This is consistent with the extensive literature on IFRS adoption by countries that generally argues that IFRS are of higher quality compared to many countries' domestic accounting standards (DeFond, Hu, Hung, and Li 2011; Khurana and Michas 2011; Yip and Young 2012). The variable *POST_IFRS* is an indicator variable equal to 1 in the years in which IFRS use is mandatory for all publicly traded firms in a country, and 0 otherwise (Deloitte 2003, 2005, 2006, 2007, 2009).

To test H2, we measure audit quality (*AUD_QUAL*) using the audit profession development measure introduced by Michas (2011). *AUD_QUAL* captures 13 individual components of four general aspects of the audit profession: (1) auditor education, (2) auditing standards, (3) auditor independence, and (4) auditor oversight, using the World Bank's Reports on Standards and Codes (ROSC) and surveys from the International Federation of Accountants (IFAC). See Appendix C for additional details on the ROSC and IFAC reports and the calculation of *AUD_QUAL*. Given that the Bank does not specify which audit firm or organization must conduct the audit for a specific project, we believe that a country-level measure of audit quality best reflects the Bank's overall view of the *a priori* level of audit quality that is likely to be implemented for a specific project in a country. We also exclude country fixed effects in this analysis, as *AUD_QUAL* by construction does not vary over time within countries.

For our tests of H3, we employ three variables that proxy for the geo-political influence of the U.S. on loan amounts provided by the Bank. First, *UN_VOTING* is compiled by the U.S. Department of State (2014a) and measures the percentage of occurrences each year in the U.N. General Assembly in which a country's vote aligns with that of the U.S. Andersen et al. (2006) and Kilby (2009) also use this to proxy for U.S. geo-political interest alignment. We split this variable into high and low voting alignment subsamples based on the threshold of 50 percent voting alignment. Second, *UN_SC_MEMBER* is equal to 1 in the years a country is serving as a temporary member on the United Nations Security Council (UNSC). This measure is also used by Dreher et al. (2009) and reflects U.S. alignment based on Kuziemko and Werker (2006), who find that foreign aid provided to a country by the U.S. increases when a country's representatives serve on the UNSC. Our final variable follows Neumayer (2003b), who measures the minimum geographic distance from a country's capital city to one of the three following capital cities: New York, New York, Rotterdam, The Netherlands, and Tokyo, Japan. We augment this measure to focus on U.S. influence, and calculate the variable *DISTANCE* as the geographical distance between a country's capital city and Washington, DC. We also split this measure into high and low subsamples based on the median value of 4,678 miles across our 42 sample countries. Some sample sizes are smaller for our tests of H3 due to data availability for these additional partitioning test variables.

Finally, for our tests of H4, we employ the "control of corruption" index compiled by Kaufmann, Kraay, and Mastruzzi (2009). We multiply this index by −1 so that higher levels of our variable *CORRUPT* indicate higher levels of corruption in a country. We split this measure into high and low subsamples based on the median value of 0.0 in Kaufmann et al. (2009).[13]

We also identify and control for a multitude of country-level characteristics that are commonly included in prior studies that investigate the determinants of international aid (Alesina and Weder 2002; Dreher et al. 2009; Neumayer 2003a). Although many of our control variables, as defined in Appendix A, are highly correlated with one another, the coefficient estimates on our test variables are not affected because these control variables are not highly correlated with our test variables. The directional prediction on control variable coefficients is based on the studies discussed in Appendix A.

Sample

Table 1 shows that 318 IBRD-eligible country-year observations receive Bank lending from 1999–2008.[14] We then exclude 60 observations for which data are not available for our control variables, leaving a final sample of 258 country-year observations over the years 1999 through 2008 representing 42 different countries, which forms our base sample. We use the base sample to test mandated IFRS adoption because the data are available for all country-year observations in the base sample. We then exclude 165 country-years missing firm-level data in Compustat Global necessary to calculate *OPACITY* and *EARN_MGMT*, and are left with a sample of 93 country-year observations comprised of 15 countries for these analyses. To form our sample for tests using *IFRS_DIFF*, we exclude 72 observations from the base sample that are missing IFRS differences data. Further, given that these differences were measured as of 2001, and that there are significant changes in accounting standards over time, we retain only observations between 1999 and 2003. The resulting *IFRS_DIFF* sample is comprised of 96 country-year observations from 1999 through 2003 from 27 countries. Finally, for our tests of audit quality, we exclude from the base sample 35 observations in countries for which no ROSC report has been issued to measure *AUD_QUAL*, and 44 observations that are more than two years after the issuance of that country's ROSC report by the Bank.[15] The final audit quality sample includes 179 country-year observations from a total of 32 countries.

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[13] While this classifies more country-year observations in our sample as being in high-corruption environments, where 45 (213) observations are classified as being in the high- (low-) corrupt subsamples, we feel it is most appropriate to use their median value as their study contains many more of the world's countries than ours (208 compared to our 42). The Kaufmann et al. (2009) measure is available for all our sample countries. If we split based on the median for only our sample countries, then many results in Table 8 show effects in both high- and low-corruption countries. However, again, this split places many countries that Kaufmann et al. (2009) consider to be high-corruption countries into the low-corruption bin. Finally, we use a different measure of corruption in the "Sensitivity Analyses" section of the study that does split on the median value for only our sample countries, with results that are very similar compared to those in Table 8.

[14] IBRD eligibility is based on per capita income levels that change from year to year. See: http://siteresources.worldbank.org/DATASTATISTICS/Resources/OGHIST.xls for an Excel spreadsheet that details eligibility cutoffs. A total of 117 country-year observations from 30 different sample countries that are eligible for IBRD loans do not receive any during our sample period. If we attempt to analyze whether accounting and audit quality affect the likelihood that a country receives a loan (of any size) using a dichotomous dependent variable, then we incur challenging conceptual and econometric issues. First, conceptually, it is highly unlikely that accounting and audit quality are "first-order" determinants of IBRD lending. In other words, the decision to loan IBRD funds to a country rests first and foremost on whether a country needs a project, and whether such a project is feasible given the construction ability and other infrastructures within a country. Second, conducting an analysis that includes country fixed effects requires us to delete the 13 countries that receive loans in all years of our sample period due to perfect collinearity. Finally, and importantly, we note that none of our sample countries fail to receive a loan during all sample years. While accounting and audit quality do likely change from year to year, we find it very difficult to believe that they change so much from one year to the next that a negative change overwhelms the first-order determinants of World Bank loans.

[15] We limit the number of years after a ROSC report to two years due to the chance that the report itself may have caused or accelerated changes in the audit profession in a country. This is a real possibility because one of the goals of the ROSC reports, created jointly by the World Bank and the IMF, is to assist countries in improving the accounting and auditing institutions in a country.

TABLE 1
Sample Selection

	n
Base Sample (and used for *POST_IFRS* analysis):	
Country-year observations eligible for and with non-zero World Bank lending data from 1999–2008	318
Less: Country-years with missing data on control variables	(60)
Final Base Sample of Country-Year Observations	258
Total Number of Countries	42
Earnings Opacity Sample:	
Base Sample	258
Less: Country-years with no firm-level data necessary to calculate country-year earnings opacity and earnings management measures	(165)
Final Earnings Opacity/Earnings Management Samples	93
Total Number of Countries	15
IFRS Differences Sample:	
Base Sample	258
Less: Country-years with missing data on IFRS differences	(72)
Less: Country-years outside the years 1999–2003	(90)
Final IFRS Differences Sample	96
Total Number of Countries	27
Audit Quality Sample:	
Base Sample	258
Less: Country-years with missing data on Audit Profession Development	(35)
Less: Country-years that are more than two years after the year of issuance of that country's ROSC report	(44)
Final Audit Profession Development Sample	179
Total Number of Countries	32

Descriptive Statistics

Table 2, Panel A provides descriptive statistics for each country for the dependent and test variables. *IFRS_YEAR* indicates the first year that *POST_IFRS* is equal to 1 for a given country. All values in this panel that vary from year to year are averaged across our 1999–2008 sample period for each country. We present loan data in $U.S. in this table before log-transformation. The overall mean value of World Bank loans is about $344 million. Venezuela ranks lowest with an average value of about $14 million, while Turkey ranks highest with an average value of about $1.6 billion.

The mean value of *OPACITY*, which ranges from 1 to 10, is 6.1 over the 15 sample countries. The Russian Federation ranks as most opaque with a value of 7.3, while Thailand ranks as least opaque with a value of 3.5. The mean value of *EARN_MGMT* is 4.3 over the 15 sample countries. Malaysia exhibits the highest level of earnings management with a value of 5.8, while Thailand again ranks lowest with a value of 2.8. The mean value of *IFRS_DIFF* is 6.9 across the 27 sample

TABLE 2

Country-Level Descriptive Statistics

Panel A: Dependent and Test Variables by Country (Number of Country-Year Observations in Parentheses)

Country	*WB_LOANS* ($thousands)		*OPACITY*		*EARN_MGMT*		*IFRS_DIFF*		*IFRS_YEAR*		*AUD_QUAL*	
Albania	16,187	(3)	NA		NA		NA		DS	(3)	0.69	(3)
Argentina	768,654	(10)	5.8	(4)	4.9	(7)	14	(5)	2009	(10)	0.28	(10)
Azerbaijan	259,500	(4)	NA		NA		NA		DS	(4)	0.29	(4)
Belarus	49,200	(3)	NA		NA		NA		DS	(3)	0.58	(3)
Bolivia	15,000	(1)	NA		NA		NA		DS	(1)	NA	
Brazil	1,561,758	(10)	6.6	(10)	4.8	(10)	11	(5)	2010	(10)	0.68	(9)
Bulgaria	190,100	(8)	NA		NA		10	(4)	2003	(8)	0.61	(5)
Cameroon	53,400	(1)	NA		NA		NA		DS	(1)	NA	
China	1,334,930	(10)	6.9	(10)	4.6	(10)	9	(5)	2007	(10)	0.81	(10)
Colombia	780,272	(10)	NA		NA		NA		DS	(10)	0.06	(7)
Dominican Rep.	55,393	(10)	NA		NA		NA		2010	(10)	NA	
Ecuador	76,269	(7)	NA		NA		NA		2010	(7)	0.22	(6)
Egypt	434,936	(7)	NA		NA		9	(3)	DS	(7)	0.45	(4)
El Salvador	99,167	(6)	NA		NA		NA		DS	(6)	0.51	(6)
Guatemala	121,526	(8)	NA		NA		0	(2)	2002	(8)	NA	
India	1,056,846	(8)	6.5	(8)	4.4	(8)	8	(5)	DS	(8)	0.61	(6)
Indonesia	779,281	(9)	6.3	(9)	3.7	(9)	4	(4)	DS	(9)	0.75	(8)
Iran	232,000	(1)	NA		NA		6	(1)	DS	(1)	NA	
Jamaica	52,150	(2)	NA		NA		NA		2002	(2)	NA	
Jordan	86,133	(9)	6.1	(5)	4.8	(6)	0	(5)	1997	(9)	0.35	(7)
Kazakhstan	130,547	(6)	NA		NA		NA		2006	(6)	0.65	(6)
Latvia	43,087	(3)	NA		NA		11	(3)	2005	(3)	0.82	(3)
Lithuania	73,127	(3)	NA		NA		11	(3)	2005	(3)	0.50	(3)
Macedonia	62,412	(7)	NA		NA		NA		2010	(7)	0.34	(4)
Malaysia	404,000	(1)	6.0	(1)	5.8	(1)	8	(1)	DS	(1)	NA	
Mauritius	26,640	(4)	NA		NA		NA		2011	(4)	0.27	(2)
Mexico	1,119,611	(10)	5.4	(10)	3.6	(10)	1	(5)	2012	(10)	0.46	(8)
Morocco	218,759	(9)	NA		NA		13	(4)	DS	(9)	0.55	(5)
Pakistan	231,286	(6)	5.9	(6)	3.1	(6)	4	(1)	DS	(6)	0.75	(5)
Papua New Guinea	63,207	(3)	NA		NA		0	(3)	1997	(3)	NA	
Paraguay	43,800	(5)	NA		NA		NA		DS	(5)	0.82	(5)
Peru	272,371	(9)	6.0	(8)	5.1	(9)	1	(4)	2011	(9)	0.40	(7)
Philippines	228,158	(10)	6.2	(10)	4.2	(10)	10	(5)	2006	(10)	0.91	(5)
Romania	310,389	(9)	NA		NA		0	(5)	2005	(9)	0.69	(7)
Russian Federation	320,575	(8)	7.3	(2)	4.3	(5)	16	(5)	DS	(8)	NA	
South Africa	15,000	(1)	7.0	(1)	4.8	(1)	0	(1)	2005	(1)	0.71	(1)
Thailand	542,145	(2)	3.5	(2)	2.8	(2)	4	(2)	2013	(2)	0.79	(2)
Tunisia	150,452	(9)	NA		NA		9	(5)	DS	(9)	0.68	(8)
Turkey	1,654,509	(9)	6.0	(7)	3.9	(8)	14	(4)	2008	(9)	0.60	(9)
Ukraine	335,646	(9)	NA		NA		7	(4)	2003	(9)	0.48	(5)
Uruguay	187,543	(6)	NA		NA		NA		2006	(6)	0.28	(6)
Venezuela	14,015	(2)	NA		NA		5	(2)	2006	(2)	NA	
Mean (# Obs.)	344,523	(258)	6.1	(93)	4.3	(93)	6.9	(96)	2006	(258)	0.55	(179)

(continued on next page)

TABLE 2 (continued)

DS indicates that a country still uses domestic accounting standards as of the year 2013.

Variable Definitions:

WB_LOANS = natural log of the $U.S. value of loan commitments made by the World Bank's International Bank for Reconstruction and Development (IBRD) to a country in year *t;*

OPACITY = a measure of accounting quality as calculated in Bhattacharya et al. (2003) and is a composite of three measures of accounting quality: (1) earnings aggressiveness, (2) loss avoidance, and (3) earnings smoothing;

EARN_MGMT = a measure of earnings management following Leuz et al. (2003) and is a composite of four measures of accounting quality: (1) smoothing of reported operating earnings using accruals, (2) smoothing and the correlation between changes in accruals and operating cash flows, (3) the magnitude of accruals, and (4) small loss avoidance;

IFRS_DIFF = difference between International Accounting Standards (IAS) and a country's domestic accounting standards as compiled by Bae et al. (2008). It measures the difference between IAS and domestic GAAP in 21 key accounting rules;

IFRS_YEAR = first year of mandatory adoption of International Financial Reporting Standards as issued by the IASB; and

AUD_QUAL = average of four aspects of a country's audit profession development. Each aspect includes individual components of Auditor Education, Audit Standards, Auditor Independence, and Auditor Oversight in a country; see Appendix C for details on *AUD_QUAL.*

Panel B: Descriptive Statistics for the Four Aspects of Audit Profession Development (*AUD_QUAL*)

	Education					Standards	Independence						Oversight				
Country	**1.**	**2.**	**3.**	**4.**	**Avg.**	**5.**	**6.**	**7.**	**8.**	**9.**	**10.**	**Avg.**	**11.**	**12.**	**13.**	**Avg.**	**Sum**
Albania	0	1	1	1	0.75	1.00	1	0.33	0	1	1	0.67	1	0.00	0	0.33	8.33
Argentina	0	0	0	0	0.00	0.33	0	0.33	1	1	0	0.47	1	0.00	0	0.33	3.67
Azerbaijan	0	0	1	1	0.50	0.33	0	0.00	0	0	0	0.00	1	0.00	0	0.33	3.33
Belarus	1	1	1	1	1.00	0.67	0	0.00	0	0	0	0.00	1	0.00	1	0.67	6.67
Brazil	1	1	0	1	0.75	0.67	1	0.33	1	1	0	0.67	1	0.50	1	0.83	9.50
Bulgaria	0	1	1	1	0.75	0.33	1	0.67	0	0	1	0.53	1	0.50	1	0.83	8.50
China	0	1	1	1	0.75	0.67	1	1.00	1	1	1	1.00	1	0.50	1	0.83	11.17
Columbia	0	0	1	0	0.25	0.00	0	0.00	0	0	0	0.00	0	0.00	0	0.00	1.00
Ecuador	0	0	0	0	0.00	0.67	1	0.00	0	0	0	0.20	0	0.00	0	0.00	1.67
Egypt	0	0	1	0	0.25	1.00	1	0.00	0	0	0	0.20	1	0.00	0	0.33	4.00
El Salvador	0	0	0	1	0.25	1.00	1	0.33	0	0	1	0.47	1	0.00	0	0.33	5.33
India	0	1	1	1	0.75	0.67	1	0.00	0	0	0	0.20	1	0.50	1	0.83	7.17
Indonesia	1	1	1	1	1.00	0.33	1	0.33	1	1	0	0.87	1	1.00	1	1.00	10.67
Jordan	0	0	0	0	0.00	0.67	1	0.00	0	1	0	0.20	1	0.00	0	0.33	3.67
Kazakhstan	1	1	1	1	1.00	1.00	1	0.33	0	0	0	0.27	1	0.00	0	0.33	7.33
Latvia	1	1	1	1	1.00	1.00	1	0.67	1	1	1	0.93	1	0.00	0	0.33	10.67
Lithuania	0	1	1	1	0.75	0.33	1	0.00	0	1	1	0.60	1	0.00	0	0.33	7.33
Macedonia	0	1	1	0	0.50	0.67	0	0.00	0	0	1	0.20	0	0.00	0	0.00	3.67
Mauritius	0	0	0	0	0.00	1.00	0	0.33	0	0	0	0.07	0	0.00	0	0.00	1.33
Mexico	0	0	0	1	0.25	0.67	1	0.00	1	1	0	0.60	1	0.00	0	0.33	5.67
Morocco	1	1	1	1	1.00	0.67	1	0.00	0	0	0	0.20	1	0.00	0	0.33	6.67
Pakistan	1	1	1	0	0.75	1.00	0	0.00	1	1	1	0.60	0	1.00	1	0.67	9.00
Paraguay	0	0	1	1	0.50	1.00	1	0.67	1	1	1	0.93	1	0.50	1	0.83	10.17
Peru	0	0	0	0	0.00	0.67	1	0.33	0	0	0	0.27	1	0.00	1	0.67	4.00
Philippines	1	1	1	1	1.00	1.00	1	1.00	0	1	1	0.80	1	0.50	1	0.83	11.50
Romania	1	1	1	1	1.00	0.67	1	0.33	0	0	0	0.27	1	0.50	1	0.83	8.50
South Africa	1	1	1	1	1.00	0.67	1	0.33	1	0	1	0.67	1	0.50	0	0.00	9.50
Thailand	1	1	1	1	1.00	0.67	1	0.33	0	1	1	0.67	1	0.50	1	0.83	10.50
Tunisia	0	1	1	0	0.50	1.00	1	0.67	0	1	1	0.67	1	0.50	0	0.50	8.17

(continued on next page)

TABLE 2 (continued)

	Education					Standards	Independence						Oversight				
Country	**1.**	**2.**	**3.**	**4.**	**Avg.**	**5.**	**6.**	**7.**	**8.**	**9.**	**10.**	**Avg.**	**11.**	**12.**	**13.**	**Avg.**	**Sum**
Turkey	1	1	1	1	1.00	0.33	1	0.00	0	1	0	0.20	1	0.00	1	0.67	8.33
Ukraine	0	1	0	1	0.50	0.67	1	0.00	0	0	1	0.40	1	0.00	0	0.33	5.67
Uruguay	1	0	0	0	0.25	0.67	1	0.00	0	0	0	0.20	0	0.00	0	0.00	2.67
Means	0.41	0.63	0.69	0.66	0.59	0.69	0.78	0.26	0.28	0.47	0.44	0.44	0.81	0.22	0.41	0.46	6.73

The coding of all components is determined by answering the questions contained in Appendix C, which are divided into four basic aspects of a country's audit profession. The final measure of audit profession development is calculated by first averaging the components within each of the four aspects within each country, and then taking the average of the four aspects. SUM is the sum of the 13 individual questions that comprise Audit Profession Development in a country, and is presented in this form in this table for descriptive purposes. The value of *AUD_QUAL* in all analyses ranges from a theoretical low of 0.0 to a high of 1.0.
See Appendix C for details on each of the questions.

countries with data. The Russian Federation has the lowest accounting quality with a value of 16, while Guatemala, Jordan, Papua New Guinea, Romania, and South Africa have the highest accounting quality with a value of zero differences. The year of IFRS adoption (IAS prior to 2002) varies across countries from as early as 1997 to as recently as 2013, with 17 countries continuing to use their own accounting standards after 2012, noted by "DS" for domestic accounting standards.[16] Finally, the mean value of *AUD_QUAL* over the 32 sample countries is 0.55. Colombia ranks lowest with a value of 0.06, while Philippines ranks highest with a value of 0.91. Table 2, Panel B provides a detailed coding of the 13 *AUD_QUAL* components sorted by the four main aspects, the average for each of the four main aspects, and the overall sum for each country. We provide a country-level sum of the components for descriptive purposes in Panel B, whereas the measure of *AUD_QUAL* used in all analyses is based on the value presented in Panel A, as calculated in Michas (2011). Thus, the measure used in all analyses ranges from a theoretical minimum of 0.0 to a theoretical maximum of 1.0 for each country

Table 3 reports descriptive statistics for all variables used in our analyses over the maximum sample of country-year observations given the specific variable. We do not tabulate correlation values for brevity, but we note again that our five test variables are not highly correlated with most control variables, with no correlation above 0.40 and seven correlations above 0.30. An analysis of variance inflation factor (VIF) values for all our test variables in all regression analyses confirms that all VIF values are below 4.0, well below the 10.0 threshold in Kennedy (1992). This provides comfort that multicollinearity is not of concern in these models and does not bias the coefficients on our test variables.

Finally, we note that our test variables are not highly correlated with each other, except for changes in *OPACITY* and changes in *EARN_MGMT*, which provides confidence that we likely are measuring different constructs. The changes in *OPACITY* and changes in *EARN_MGMT* variables are correlated at 0.83 and 0.87 (Pearson and Spearman, respectively). However, these variables are never used in the same model. We also note that *IFRS_DIFF* and *POST_IFRS* are expectedly highly negatively correlated at −0.70 and −0.57 for Pearson and Spearman correlations, respectively, but again, these two variables are never used in the same model. All other test variables exhibit correlations with each other below 0.072, and are never significant at the $p < 0.20$ level, two-tailed.

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[16] While our sample period ends before 2013 due to data availability on loans and other control variables, we still code the variable *IFRS_ADOPTER* as equal to 1 for countries that eventually adopt IFRS, even if it is after the end of our sample period.

TABLE 3

Descriptive Statistics for Country-Year-Level Variables

Variable	n	Mean	Std. Dev.	25%	Median	75%
WB_LOANS ($thousands)	258	458,172	577,066	68,100	200,000	628,060
OPACITY (ranks 1–10)	93	5.17	1.23	4.67	5.33	6.00
CH_OPACITY (ranks 1–10)	93	0.19	1.52	–0.67	0.00	1.00
EARN_MGMT (ranks 1–10)	93	3.24	0.93	2.50	3.25	3.75
CH_EARN_MGMT (ranks 1–10)	93	0.08	1.01	–0.50	0.00	0.50
IFRS_DIFF	96	7.27	5.19	1.00	9.00	11.00
POST_IFRS	258	0.17	0.37	0.00	0.00	0.00
AUD_QUAL	179	0.54	0.21	0.35	0.60	0.69
IFRS_ADOPTER (%)	258	55.03	49.84	0.00	100.00	100.00
DEMOC	258	6.03	3.34	4.00	8.00	8.00
CORRUPT	258	0.40	0.41	0.19	0.40	0.72
REG_QUAL	258	–0.10	0.44	–0.43	–0.09	0.20
RULE_OF_LAW	258	–0.41	0.45	–0.79	–0.49	–0.06
POL_FREEDOM	258	59.33	21.01	42.00	64.00	75.00
HUMAN_RIGHTS	258	–5.97	1.80	–8.00	–6.00	–5.00
OPENNESS ($millions)	258	136,296	307,919	16,734	41,429	124,551
EXPORTS ($millions)	258	70,774	169,105	7,537	20,422	62,625
MORTALITY (per TH)	258	32.23	19.13	20.20	26.85	37.80
INFANT_MORT (per TH)	258	26.05	13.94	17.20	22.60	30.40
LIFE_EXP	258	70.43	3.78	68.28	71.38	72.72
PRIV_CAP ($millions)	258	261,857	532,010	22,203	74,741	232,880
BANK_CRED ($millions)	258	197,651	613,269	7,508	26,349	101,048
CRED_RATING	258	3.47	1.96	2.00	4.00	5.00
SAVINGS ($millions)	258	78,643	233,162	2,542	12,938	49,434
POPULATION (millions)	258	129.50	298.23	8.72	28.45	85.54
GDP ($millions)	258	255,018	515,521	21,268	72,475	232,534
GDP_GROWTH (%)	258	5.13	4.31	3.30	5.06	6.90
GDP_PER_CAP ($)	258	3,061	2,100	1,578	2,457	3,861
INFLATION (%)	258	9.51	12.59	3.46	6.38	10.31
LITERACY (%)	258	87.22	13.07	86.36	91.07	97.19
MILITARY_EXP ($millions)	258	1,578,223	6,244,344	1,414	11,208	78,663
US_MIL_AID ($thousands)	258	50,799	214,638	0.00	473	6,944

See Appendix A for definitions of remaining control variables.

Variable Definitions:

WB_LOANS = natural log of the $U.S. value of loan commitments made by the World Bank's International Bank for Reconstruction and Development (IBRD) to a country in year *t;*

OPACITY = a measure of accounting quality as calculated in Bhattacharya et al. (2003) and is a composite of three measures of accounting quality: (1) earnings aggressiveness, (2) loss avoidance, and (3) earnings smoothing;

CH_OPACITY = change in *OPACITY* from year *t*–1 to *t* in a country;

EARN_MGMT = a measure of earnings management following Leuz et al. (2003) and is a composite of four measures of earnings quality: (1) smoothing of reported operating earnings using accruals, (2) smoothing and the correlation between changes in accruals and operating cash flows, (3) the magnitude of accruals, and (4) small loss avoidance;

CH_EARN_MGMT = change in *EARN_MGMT* from year *t*–1 to *t* in a country;

IFRS_DIFF = difference between International Accounting Standards (IAS) and a country's domestic accounting standards, as compiled by Bae et al. (2008). It measures the difference between IAS and domestic GAAP in 21 key accounting rules;

POST_IFRS = an indicator variable equal to 1 in the years in which IFRS use is mandatory for all publicly traded firms in a country, and 0 otherwise;

(continued on next page)

TABLE 3 (continued)

AUD_QUAL = average of four aspects of a country's audit profession development. Each aspect includes individual components of Auditor Education, Audit Standards, Auditor Independence, and Auditor Oversight in a country; see Appendix C for details on *AUD_QUAL*.

IV. MAIN RESULTS

Accounting/Audit Quality and World Bank Lending

Table 4 reports the results of our empirical model in Equation (1) that tests H1 and H2 for all our separate test variables. In all tables, bold figures indicate our test variables, and combined coefficients in later tables, of interest, and all significance levels are based on two-tailed tests. P-values are also provided in parentheses for the test variables. The "Changes" ("Levels") headings in all tables indicate that all variables in the respective models are the year *t* less year *t*–1 (year *t*) calculated value.[17] The coefficients on *OPACITY* and *EARN_MGMT* are both negative and significant at $p < 0.05$, indicating that a positive change in these variables is associated with a negative change in the amount of lending provided by the Bank; i.e., decreases in earnings quality are associated with decreases in lending, consistent with H1. Further, the third (fourth) model shows that the coefficient on *IFRS_DIFF* (*POST_IFRS*) is negative (positive) and significant at $p < 0.05$. This indicates that higher levels of accounting quality are positively associated with lending levels, also consistent with H1, with an important caveat about our *POST_IFRS* analyses overall that we explain in the "Sensitivity Analyses" section of the study. Finally, in the fifth and final model, *AUD_QUAL* is positively and significantly ($p < 0.05$) associated with loan amounts, as predicted in H2. Model R^2s are relatively high at between 59.2 and 78.4 percent.[18] We also include our *POST_IFRS* variable as a control in our audit quality model to control for accounting quality, where *POST_IFRS* is the only accounting quality variable available for all countries in our audit quality sample.

The impacts of accounting and audit quality are also economically meaningful. When considering our test variables in Table 4, an increase in one rank for *OPACITY* (*EARN_MGMT*) results in a 21.4 (28.7) percent decrease in loan amounts. Further, an increase of one difference between a country's domestic accounting standards and IFRS results in a 7.6 percent decrease in total loan amounts, and countries that mandate IFRS use receive, on average, 38.6 percent more in loan amounts compared to countries that use domestic accounting standards. Finally, for each 0.1 increase in *AUD_QUAL*, where this variable ranges from 0.0 to 1.0, countries receive 12.7 percent more, on average, in loan amounts.[19]

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[17] We have suppressed the reporting of the intercept in all models that include country fixed effects due to the random choosing of a "base" country that the intercept reflects. We thank an anonymous reviewer for this suggestion.

[18] For robustness, we delete each sample country, one at a time, and re-perform our analysis in Table 4 (a total of 121 new models). We find that all our results are robust at $p < 0.10$ (two-tailed), except in seven cases where results are robust at the 0.20 level (two-tailed). Consequently, our test variables are not strongly influenced by the deletion of any specific country.

[19] We note that all our results for *OPACITY* and *EARN_MGMT* are very similar in all analyses, in terms of the signs and significance levels of the coefficients, if we use a "levels" model instead of a "changes" model for these test variables. Further, given that our dependent variable is in natural log form, the interpretation of the coefficients on our test variables is that the coefficient is the proportionate change (or percentage change if multiplied by 100) in the dependent variable for each one-unit increase in the test variable. We calculate these percentage changes as follows: We first run "levels" rather than "changes" analyses on *OPACITY* and *EARN_MGMT* so that we can obtain the percentage effect for a one-rank increase in each of these variables. The coefficients on these variables in this levels analysis are –0.214 and –0.287, respectively. For our *IFRS_DIFF* and *POST_IFRS* variables, we simply use the coefficients presented in Table 4. For our *AUD_QUAL* variable, we divide the model coefficient by 10 (as this variable ranges from 0.0 to 1.0) so that we can obtain the proportionate change in loan amounts based on a one-unit increase in *AUD_QUAL*. This effectively treats this variable as ranging from 0.0 to 10.0.

TABLE 4

Accounting Quality, Audit Quality, and World Bank Lending Commitments (H1, H2)

Variable	Pred.	Dependent Variable = *WB_LOANS*				
		Changes	Changes	Levels	Levels	Levels
Test Variables:						
OPACITY	–	−0.78**				
EARN_MGMT	–		−0.89**			
IFRS_DIFF	–			−0.08***		
POST_IFRS	+				0.38**	
AUD_QUAL	+					1.27**
(p-value)		(0.012)	(0.030)	(0.001)	(0.014)	(0.032)
Control Variables:						
IFRS_ADOPTER	?	−0.09	−10.58*	0.53***	19.89***	0.40
POST_IFRS	+					0.58
DEMOC	+	−1.48**	−1.77***	0.13***	−0.35	0.04
CORRUPT	?	6.81	16.24***	−1.39***	0.11	−0.96
REG_QUAL	?	−11.26**	−4.95	0.26	−0.29	−1.14**
RULE_OF_LAW	?	8.75	8.55	−1.05**	1.34***	0.68
POL_FREEDOM	+	0.50***	0.32*	−0.01	−0.02	0.00
HUMAN_RIGHTS	–	−0.71	−0.81	−0.02	0.05	−0.16**
OPENNESS	–	−22.72	0.92	−4.18***	−0.46	−4.29**
EXPORTS	+	22.51	6.00	4.06***	1.09	4.91***
MORTALITY	+	−2.36	−0.47	0.09	−0.16*	0.07
INFANT_MORT	+	−4.60	−4.30	−0.07	0.25*	−0.06
LIFE_EXP	–	−15.95	2.34	0.13*	0.26	0.16**
PRIV_CAP	–	−17.06	−33.17	−0.18	−4.49***	−9.72***
BANK_CRED	–	−7.50	−3.72	0.60***	0.09	−0.27
CRED_RATING	?	1.65	1.84	−0.10	−0.21***	0.36***
SAVINGS	–	−99.23***	−63.45**	−2.88	−7.32***	−3.93
POPULATION	+	−43.80	−82.62	10.87	−4.81*	34.48***
GDP	+	35.90	91.22	0.21	8.97***	−20.34***
GDP_GROWTH	?	11.29	−7.09	2.97	−1.24	3.27
GDP_PER_CAP	–	−34.60	85.51	0.27	1.53	34.43***
INFLATION	?	17.99	−1.92	1.58*	0.79	−2.67***
LITERACY	–	−0.14	−0.42	0.01	−0.12**	0.01
MILITARY_EXP	–	−8.39*	−11.08	−0.33	0.71***	0.08
US_MIL_AID	+	0.15	0.74**	0.01	0.00	0.07***
Country Fixed Effects		Yes	Yes	No	Yes	No
Year Fixed Effects		Yes	Yes	Yes	Yes	Yes
n		93	93	96	258	179
Number of Countries		15	15	27	42	32
OLS R^2		59.2%	61.8%	62.0%	78.4%	74.4%

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*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.

All models use Generalized Least Squares (GLS), which corrects for panel-specific AR1 autocorrelation within countries, as well as heteroscedasticity. Coefficients on country and year indicator variables are not reported for brevity.

See Appendix A for definitions of remaining control variables.

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TABLE 4 (continued)

Variable Definitions:
WB_LOANS = natural log of the \$U.S. value of loan commitments made by the World Bank's International Bank for Reconstruction and Development (IBRD) to a country in year *t;*
OPACITY = a measure of accounting quality as calculated in Bhattacharya et al. (2003) and is a composite of three measures of accounting quality: (1) earnings aggressiveness, (2) loss avoidance, and (3) earnings smoothing;
EARN_MGMT = a measure of earnings management following Leuz et al. (2003) and is a composite of four measures of earnings quality: (1) smoothing of reported operating earnings using accruals, (2) smoothing and the correlation between changes in accruals and operating cash flows, (3) the magnitude of accruals, and (4) small loss avoidance;
IFRS_DIFF = difference between International Accounting Standards (IAS) and a country's domestic accounting standards, as compiled by Bae et al. (2008). It measures the difference between IAS and domestic GAAP in 21 key accounting rules;
POST_IFRS = an indicator variable equal to 1 in the years in which IFRS use is mandatory for all publicly traded firms in a country, and 0 otherwise; and
AUD_QUAL = average of four aspects of a country's audit profession development. Each aspect includes individual components of Auditor Education, Audit Standards, Auditor Independence, and Auditor Oversight in a country; see Appendix C for details on *AUD_QUAL.*

U.S. Influence, World Bank Lending, and Accounting/Audit Quality

Table 5 presents our results for our five test variables after splitting our samples based on voting alignment with the United States in the U.N. General Assembly (H3). The coefficients on our main test variables represent the effect of each variable on Bank loans in countries where voting alignment is low. All main coefficients on our test variables retain the same signs as in Table 4, and all are statistically significant at $p < 0.05$ or beyond, except for the coefficient on *IFRS_DIFF,* which is not significant. These results indicate that accounting and audit quality are significantly associated with Bank lending in countries that exhibit relatively low alignment with the U.S. on U.N. voting issues.

The variable *INTERACTION* in all models indicates the interaction between *HIGH_UN_VOTING* and our five test variables in all respective analyses (for example, *HIGH_UN_VOTING* * *OPACITY*). Consequently, the combined coefficient (*HIGH_UN_VOTING* + *INTERACTION*) for each main test variable, along with their respective F-statistics (all in bold), represent the association between our accounting and audit quality main variables and lending in countries where voting alignment is relatively *high,* measured as greater than 50 percent in a year. None of the combined coefficients in models one through four are significant at conventional levels (given the insignificant F-tests), indicating that accounting quality does not play a role in lending for countries where voting alignment is high. However, the combined coefficient for *AUD_QUAL* + *INTERACTION* is positive and significant, indicating that audit quality does play a role in both high and low voting alignment countries.

Overall, the results in Table 5 provide initial evidence that accounting quality is evaluated in a politically strategic manner when loan amounts are considered. The evidence implies that accounting quality is not considered when a country is more aligned with United States interests around the world in terms of voting in the U.N. General Assembly. This suggests that Bank lenders may ignore accounting quality in the lending decision and reward countries with IBRD loan amounts when a country's voting is more aligned with U.S. interests. However, this does not seem to be the case for audit quality because there is a positive and significant association in both low- and high-alignment countries.

Table 6 presents a similar analysis as Table 5, except that we replace our voting alignment variable with our variable that indicates whether a country has been elected as a temporary member of the U.N. Security Council (*UN_SC_MEMBER*).[20] Except for two differences, results are almost

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[20] We do not include a variable for permanent Security Council membership as there are only two of these countries in our sample, China and Russia.

TABLE 5

Voting Alignment with U.S. in the United Nations, Accounting and Audit Quality, and World Bank Lending Commitments

(H3)

		Dependent Variable = *WB_LOANS*				
Variable	**Pred.**	**Changes**	**Changes**	**Levels**	**Levels**	**Levels**
HIGH_UN_VOTING	?	−1.21	−1.20	−0.11	−0.19	−0.59
OPACITY	−	**−1.40*** **				
EARN_MGMT	−		**−1.81*** **			
IFRS_DIFF	−			**0.09**		
POST_IFRS	+				**0.47** **	
AUD_QUAL	+					**1.56** **
(p-value)		(0.000)	(0.000)	(0.206)	(0.022)	(0.050)
INTERACTION		2.18***	2.56**	−0.08	0.01	1.53*
F-test in high *UN_VOTING* Countries	ns	**1.56**	**0.68**	**0.02**	**0.80**	**13.07*** **
(p-value)	ns	(0.216)	(0.411)	(0.875)	(0.373)	(0.003)
IFRS_ADOPTER	?	−12.50***	−11.55***	−0.37	36.89***	1.00***
POST_IFRS	+					0.30**
DEMOC	+	−1.15**	−1.54***	0.29***	−0.08	−0.04
CORRUPT	?	7.55	10.25	0.74	0.69	−0.56
REG_QUAL	?	−7.02	−4.59	0.56	−0.46	0.55
RULE_OF_LAW	?	5.61	0.35	0.05	1.40*	0.62
POL_FREEDOM	+	0.53***	0.31*	−0.01	−0.04*	0.00
HUMAN_RIGHTS	−	−0.54	−0.57	−0.30**	−0.02	0.02
OPENNESS	−	−29.01**	−23.55*	1.60	1.38	−1.63
EXPORTS	+	28.81**	28.34*	−1.66	−0.08	0.99
MORTALITY	+	−4.03	−2.34	−0.18	−0.29**	0.14**
INFANT_MORT	+	−4.17	−8.03	0.37	0.58***	−0.17**
LIFE_EXP	−	−22.83	−22.53	0.35	1.18**	0.21*
PRIV_CAP	−	−2.39	4.13	−12.01**	−7.12***	−12.43***
BANK_CRED	−	−5.71	−2.26	−0.29	0.02	−0.47**
CRED_RATING	?	2.11	1.36	−0.48***	−0.21**	0.14
SAVINGS	−	−110.12***	−95.23***	3.27	−9.65***	−1.14
POPULATION	+	133.71	−19.52	7.66	11.11***	65.86
GDP	+	−21.71	19.91	1.66	−5.26	−50.94
GDP_GROWTH	?	14.97	−1.26	9.62**	1.88	0.56
GDP_PER_CAP	−	23.03	−18.88	8.10	20.62***	66.17
INFLATION	?	20.85*	2.30	−0.04	0.50	−0.36
LITERACY	−	−0.19	−0.45	0.08	−0.10	−0.02
MILITARY_EXP	−	−6.46	−8.41	−0.24	0.22	0.32
US_MIL_AID	+	0.13	0.05*	−0.04	0.01	−0.01***
Intercept				−0.53		−71.43
Country Fixed Effects		Yes	Yes	No	Yes	No
Year Fixed Effects		Yes	Yes	Yes	Yes	Yes
n		89	89	71	165	130
Number of Countries		12	12	17	20	20
OLS R^2		56.4%	61.2%	79.2%	75.4%	75.0%

(continued on next page)

TABLE 5 (continued)

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*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.
All models use Generalized Least Squares (GLS), which corrects for panel-specific AR1 autocorrelation within countries, as well as heteroscedasticity. Coefficients on country and year indicator variables are not reported for brevity.
See Appendix A for definitions of remaining control variables.

Variable Definition:
WB_LOANS = natural log of the $U.S. value of loan commitments made by the World Bank's International Bank for Reconstruction and Development (IBRD) to a country in year *t;*
UN_VOTING = alignment, in percentage terms, of a country's voting in the United Nations General Assembly with United States voting (U.S. Department of State 2014a). A country is classified as high alignment when its votes align with the U.S. 50 percent or more in a year, low alignment otherwise;
OPACITY = a measure of accounting quality as calculated in Bhattacharya et al. (2003) and is a composite of three measures of accounting quality: (1) earnings aggressiveness, (2) loss avoidance, and (3) earnings smoothing;
EARN_MGMT = a measure of earnings management following Leuz et al. (2003) and is a composite of four measures of earnings quality: (1) smoothing of reported operating earnings using accruals, (2) smoothing and the correlation between changes in accruals and operating cash flows, (3) the magnitude of accruals, and (4) small loss avoidance;
IFRS_DIFF = difference between International Accounting Standards (IAS) and a country's domestic accounting standards, as compiled by Bae et al. (2008). It measures the difference between IAS and domestic GAAP in 21 key accounting rules;
POST_IFRS = an indicator variable equal to 1 in the years in which IFRS use is mandatory for all publicly traded firms in a country, and 0 otherwise;
AUD_QUAL = average of four aspects of a country's audit profession development. Each aspect includes individual components of Auditor Education, Audit Standards, Auditor Independence, and Auditor Oversight in a country; see Appendix C for details on *AUD_QUAL;* and
INTERACTION = in all models, indicates the interaction between *HIGH_UN_VOTING* and our five respective test variables (for example, *HIGH_UN_VOTING * OPACITY*).

exactly the same compared to Table 5 in terms of the signs and significance levels on all main and combined coefficients. The first difference is that the coefficient on *IFRS_DIFF* is negative and significant as predicted, whereas it is not significant in Table 5. Second, the analysis of audit quality in this table aligns with our predictions in that audit quality is now positively associated with loans only in countries that are *not* temporary Security Council members. Overall, the results in this table indicate that temporary U.N. Security Council membership, which changes for a country every two years, is associated with the evaluation and use of accounting and audit quality in a country by the Bank. Consequently, we provide more evidence that accounting quality, and now also audit quality, are used in a political fashion by Bank lenders who are influenced by U.S. policy making and interests.

In Table 7, our proxy for U.S. influence is the geographic distance between a country's capital city and Washington, DC, split into high and low distance samples based on the sample country median using our *HIGH_DISTANCE* partitioning variable. We note that predicted signs for the main and combined coefficients are now "reversed" compared to previous tables to reflect no association, as we predict countries closer to Washington, DC are more aligned with U.S. interests. Further, we now predict a negative/positive and significant association for our combined coefficients (negative for *OPACITY, EARN_MGMT,* and *IFRS_DIFF,* and positive for *POST_IFRS* and *AUD_QUAL*), as in Table 4. Our results again provide evidence that U.S. influence plays a role when the Bank considers accounting and audit quality, as our results are consistent with those in Table 6. Consequently, we provide evidence that the Bank does not consider accounting and auditing in countries that are closer to the U.S. geographically, but does in countries that are geographically more distant.

Corruption, World Bank Lending, and Accounting/Audit Quality

Finally, Table 8 presents our tests of H4 on whether accounting and audit quality are affected by the level of overall corruption in a country. We split our samples into high- and low-corruption

TABLE 6

U.N. Security Council Membership, Accounting and Audit Quality, and World Bank Lending Commitments
(H3)

		Dependent Variable = *WB_LOANS*				
Variable	**Pred.**	**Changes**	**Changes**	**Levels**	**Levels**	**Levels**
UN_SC_MEMBER	?	0.09	0.25	0.01	−0.07	1.10
OPACITY	−	**−1.46*****				
EARN_MGMT	−		**−1.68*****			
IFRS_DIFF	−			**−0.10*****		
POST_IFRS	+				**0.44****	
AUD_QUAL	+					**1.48****
(p-value)		(0.000)	(0.003)	(0.001)	(0.039)	(0.012)
INTERACTION		1.62**	2.04*	0.03	0.34	−2.41
F-test in *UN_SC_MEMBER* Countries	ns	**0.06**	**0.20**	**1.65**	**2.30**	**0.35**
(p-value)		(0.811)	(0.656)	(0.198)	(0.129)	(0.555)
IFRS_ADOPTER	?	−13.18	−9.52*	0.31	20.00***	0.40
POST_IFRS	+					0.83*
DEMOC	+	−1.80***	−2.07***	−0.02	0.02	0.07
CORRUPT	?	5.05	9.30	−1.28*	0.44	−1.20
REG_QUAL	?	−6.45	−3.97	−0.07	−0.01	−1.31**
RULE_OF_LAW	?	−2.72	8.11	−0.84	0.76	0.44
POL_FREEDOM	+	0.54**	0.34*	0.02	−0.01	0.00
HUMAN_RIGHTS	−	−0.65	−0.86*	0.05	0.01	−0.15*
OPENNESS	−	−23.15	−14.71	−5.03**	0.08	−4.47**
EXPORTS	+	17.33	18.32	5.01**	0.57	5.15**
MORTALITY	+	−2.14	1.94	0.05	−0.13	0.05
INFANT_MORT	+	−3.91	−7.01	−0.07	0.18	−0.02
LIFE_EXP	−	−0.35	2.15	0.08*	0.23	0.16**
PRIV_CAP	−	−15.87	−30.14	−6.99	−6.24***	−10.27***
BANK_CRED	−	3.31	2.76	0.47	−0.13	−0.28
CRED_RATING	?	1.48	1.57	−0.26	−0.19**	0.33***
SAVINGS	−	−95.01***	−76.58**	−7.29**	−6.27***	−3.80
POPULATION	+	−64.39	−75.84	14.65***	−5.40**	38.61***
GDP	+	71.80	83.04	0.12	10.08***	−24.06***
GDP_GROWTH	?	−3.35	−3.01	3.37	−1.97	2.77
GDP_PER_CAP	−	−70.56	−81.92	14.79***	1.34	38.63***
INFLATION	?	13.06	−3.85	−0.33	1.08*	−2.79***
LITERACY	−	−0.30	−0.59	−0.01	−0.20***	0.01
MILITARY_EXP	−	−7.90	−12.29	−0.05*	0.75**	0.07
US_MIL_AID	+	1.26*	0.94**	−0.02	0.00	0.08***
Intercept				23.97		−34.85***
Country Fixed Effects		Yes	Yes	No	Yes	No
Year Fixed Effects		Yes	Yes	Yes	Yes	Yes
n		93	93	96	258	179
Number of Countries		15	15	27	42	32
OLS R^2		61.3%	64.2%	62.7%	78.5%	74.6%

(continued on next page)

TABLE 6 (continued)

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*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.
All models use Generalized Least Squares (GLS), which corrects for panel-specific AR1 autocorrelation within countries, as well as heteroscedasticity. Coefficients on country and year indicator variables are not reported for brevity.
See Appendix A for definitions of remaining control variables.

Variable Definitions:
WB_LOANS = natural log of the $U.S. value of loan commitments made by the World Bank's International Bank for Reconstruction and Development (IBRD) to a country in year *t;*
UN_SC_MEMBER = 1 in years in which a country has been elected as a temporary member to the U.N. Security Council;
OPACITY = a measure of accounting quality as calculated in Bhattacharya et al. (2003) and is a composite of three measures of accounting quality: (1) earnings aggressiveness, (2) loss avoidance, and (3) earnings smoothing;
EARN_MGMT = a measure of earnings management following Leuz et al. (2003) and is a composite of four measures of earnings quality: (1) smoothing of reported operating earnings using accruals, (2) smoothing and the correlation between changes in accruals and operating cash flows, (3) the magnitude of accruals, and (4) small loss avoidance;
IFRS_DIFF = difference between International Accounting Standards (IAS) and a country's domestic accounting standards, as compiled by Bae et al. (2008). It measures the difference between IAS and domestic GAAP in 21 key accounting rules;
POST_IFRS = an indicator variable equal to 1 in the years in which IFRS use is mandatory for all publicly traded firms in a country, and 0 otherwise;
AUD_QUAL = average of four aspects of a country's audit profession development. Each aspect includes individual components of Auditor Education, Audit Standards, Auditor Independence, and Auditor Oversight in a country; see Appendix C for details on *AUD_QUAL;* and
INTERACTION = in all models, indicates the interaction between *UN_SC_MEMBER* and our five respective test variables (for example, *UN_SC_MEMBER* $*$ *OPACITY*).

countries. Similar to Table 7, we expect significant associations in *HIGH_CORRUPT* countries, as represented by the combined coefficients of our test variables and *HIGH_CORRUPT*. Given the insignificant results on all of our main test variables, our results provide evidence that accounting and audit quality are not considered by the Bank when making loan amount decisions in countries with relatively low corruption levels. Conversely, accounting quality does play a role, in the expected direction, in high-corruption countries, as shown by the significant F-tests on the first four combined coefficients. However, audit quality is not significant in either low- or high-corruption countries at conventional significance levels.

Overall, our results in Tables 5 through 8 provide evidence of an interesting interplay between accounting/audit quality in a country and other factors that seem to take precedence over accounting and audit quality in Bank lending behavior in certain instances. Our results suggest that as a country's geo-political interests become more aligned with those of the U.S., the roles of accounting and audit quality are diminished, and perhaps even eliminated. Further, our results indicate a certain level of trust in lending to countries that exhibit overall low levels of corruption in that accounting and audit quality are not significant determinants of loan amounts in these countries. However, accounting quality, but not audit quality, does play a significant role when overall corruption is high.

Our results reflect inconsistencies with the information disseminated by the World Bank itself. The Bank's published literature and information on its website generally suggest that loans are made only on the basis of need, while also considering the levels of accounting and audit quality and other country institutional factors. However, we provide evidence that accounting and audit quality influence lending only in the absence of political factors, and only in countries with high levels of corruption.

Sensitivity Analyses

We log transform our dependent variable and all of our dollar value control variables, given that this is the model specification used most often in prior literature that analyzes international

TABLE 7

Distance from Washington, DC, Accounting and Audit Quality, and World Bank Lending Commitments (H3)

		Dependent Variable = *WB_LOANS*				
Variable	**Pred.**	**Changes**	**Changes**	**Levels**	**Levels**	**Levels**
HIGH_DISTANCE	?	−2.49	−2.26	−0.73	−13.58***	−4.11***
OPACITY	ns	**0.00**				
EARN_MGMT	ns		**−1.54**			
IFRS_DIFF	ns			**−0.04**		
POST_IFRS	ns				**0.63***	
AUD_QUAL	ns					**−1.02**
(p-value)		(0.995)	(0.743)	(0.246)	(0.087)	(0.113)
INTERACTION		−2.69***	−3.33***	−0.08*	−0.31	6.01***
F-test in *HIGH_DISTANCE* Countries	− − − + +	**25.28***	**20.25***	**18.46***	**3.89**	**23.69***
(p-value)		(0.000)	(0.000)	(0.000)	(0.048)	(0.000)
IFRS_ADOPTER	?	2.04	−11.31	0.01	20.83***	0.45**
POST_IFRS	+					0.90***
DEMOC	+	−2.19***	−2.16***	−0.02	−0.02	0.01
CORRUPT	?	2.95	14.30**	−1.63***	0.19	−1.15**
REG_QUAL	?	−4.38	−3.55	−1.04**	−0.26	−0.45
RULE_OF_LAW	?	−4.44	6.15	1.22*	1.17**	1.58***
POL_FREEDOM	+	0.49**	0.44**	−0.02*	−0.02	0.00
HUMAN_RIGHTS	−	−0.53	−0.90*	−0.02	0.06	−0.27***
OPENNESS	−	−18.66	−15.14	−54.22	−0.75	−2.55*
EXPORTS	+	20.77	22.85*	1.02***	1.14	3.23***
MORTALITY	+	−4.91	−3.39	0.03	−0.17*	0.06
INFANT_MORT	+	1.83	0.09	−0.03	0.25**	−0.01
LIFE_EXP	−	2.82	1.80	−0.31***	0.22	0.03
PRIV_CAP	−	−13.71	−35.76*	−9.33***	−4.16***	−9.35***
BANK_CRED	−	2.41	0.93	0.00	0.11	−0.15
CRED_RATING	?	1.65	1.58	0.45***	−0.20***	0.38***
SAVINGS	−	−92.40***	−76.99***	43.22	−6.79***	−0.64
POPULATION	+	−46.11	−36.46	10.62***	−5.33**	34.63***
GDP	+	54.33	43.43	−1.33***	8.26***	−24.81***
GDP_GROWTH	?	1.35	11.90	1.50	−1.31	3.20
GDP_PER_CAP	−	−3.19	−42.26	10.40***	1.46	34.99***
INFLATION	?	2.78	−4.67	0.98	0.65	−0.84*
LITERACY	−	−0.49	−0.50	0.04***	−0.14**	0.03**
MILITARY_EXP	−	−4.31	−9.27	−0.04	0.67***	0.12**
US_MIL_AID	+	−0.08	−0.03**	−0.01	0.00	0.03**
Intercept				9.63		−34.22***
Country Fixed Effects		Yes	Yes	No	Yes	No
Year Fixed Effects		Yes	Yes	Yes	Yes	Yes
n		93	93	96	258	179
Number of Countries		15	15	27	42	32
OLS R^2		64.3%	67.4%	62.1%	78.5%	74.8%

(continued on next page)

TABLE 7 (continued)

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*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.
All models use Generalized Least Squares (GLS), which corrects for panel-specific AR1 autocorrelation within countries, as well as heteroscedasticity. Coefficients on country and year indicator variables are not reported for brevity.
See Appendix A for definitions of remaining control variables.

Variable Definitions:
WB_LOANS = natural log of the $U.S. value of loan commitments made by the World Bank's International Bank for Reconstruction and Development (IBRD) to a country in year *t;*
DISTANCE = geographical distance, in miles, from a country's capital city to Washington, DC. We classify this variable into high and low partitions based on the median value for all countries in our largest sample;
OPACITY = a measure of accounting quality as calculated in Bhattacharya et al. (2003) and is a composite of three measures of accounting quality: (1) earnings aggressiveness, (2) loss avoidance, and (3) earnings smoothing;
EARN_MGMT = a measure of earnings management following Leuz et al. (2003) and is a composite of four measures of earnings quality: (1) smoothing of reported operating earnings using accruals, (2) smoothing and the correlation between changes in accruals and operating cash flows, (3) the magnitude of accruals, and (4) small loss avoidance;
IFRS_DIFF = difference between International Accounting Standards (IAS) and a country's domestic accounting standards, as compiled by Bae et al. (2008). It measures the difference between IAS and domestic GAAP in 21 key accounting rules;
POST_IFRS = an indicator variable equal to 1 in the years in which IFRS use is mandatory for all publicly traded firms in a country, and 0 otherwise;
AUD_QUAL = average of four aspects of a country's audit profession development. Each aspect includes individual components of Auditor Education, Audit Standards, Auditor Independence, and Auditor Oversight in a country; see Appendix C for details on *AUD_QUAL;* and
INTERACTION = in all models, indicates the interaction between *HIGH_DISTANCE* and our five respective test variables (for example, *HIGH_DISTANCE* ∗ *OPACITY*).

lending and grant aid issuance. However, some studies also analyze the number of loans (projects) made to a country in a year (Dreher et al. 2009). We find that our results are robust, in terms of the signs and significance levels on all test variables and combined coefficients, to this change except in two out of 25 models in Tables 4 through 8. These exceptions are that *POST_IFRS* is not significantly associated with loans in either low- or high-distance countries in Table 7, and that changes in *OPACITY* are not significant in either low- or high-corruption countries in Table 8.

Second, we test the robustness of our audit quality measure by replacing *AUD_QUAL* with a variable used in Bhattacharya et al. (2003) and originally compiled by Saudagaran and Diga (1997) that measures the number of auditors in a country per 100,000 people. This variable is available for 11 of our sample countries. We find that all our results for audit quality in Tables 4 through 8 are robust to this change. The only possible exception is in our *HIGH_DISTANCE* analysis in Table 7, where the significance level in high-distance countries is only $p = 0.096$ using a two-tailed test.

Third, we also test our U.S. alignment hypothesis (H3) using our *DEMOC* control variable, where we split countries into high- and low-democracy countries based on the median value of 7.0 for our 42 sample countries. Results using this variable show that accounting and audit quality are associated with loan amounts, but only in low-democracy countries. This result is robust to all of our test variables except *POST_IFRS*. Overall, this analysis is consistent with Alesina and Dollar (2000) and Neumayer (2003a), who show that aid is less likely to be given to undemocratic governments, including, but not limited to, when the U.S. is the specific donor.

Fourth, we perform a robustness test replacing the Kaufmann et al. (2009) measure of corruption with a corruption variable compiled by the World Bank. This variable captures "informal payments to public officials" and is measured as "the percentage of firms expected to make informal payments to public officials to 'get things done' with regard to customs, taxes, licenses, regulations, services and the like" (International Finance Corporation [IFC] 2014, 22).

TABLE 8

Level of Corruption in a Country, Accounting and Audit Quality, and World Bank Lending Commitments
(H4)

		Dependent Variable = *WB_LOANS*				
Variable	**Pred.**	**Changes**	**Changes**	**Levels**	**Levels**	**Levels**
HIGH_CORRUPT	?	0.77	0.88	2.23**	0.22	0.51
OPACITY	ns	**−0.14**				
EARN_MGMT	ns		**0.17**			
IFRS_DIFF	ns			**0.12**		
POST_IFRS	ns				**0.10**	
AUD_QUAL	ns					**0.21**
(p-value)		(0.832)	(0.847)	(0.160)	(0.718)	(0.835)
INTERACTION		−1.17	−1.58	−0.22**	0.42	−0.84
F-test in *HIGH_CORRUPT* Countries	− − − + +	**11.95*****	**7.97*****	**12.88*****	**10.10*****	**1.80**
(p-value)		(0.001)	(0.004)	(0.000)	(0.002)	(0.179)
IFRS_ADOPTER	?	−18.01*	−10.99**	−0.07	22.39***	−0.38*
POST_IFRS	+					1.02***
DEMOC	+	−2.05***	−2.19***	0.06	−0.02	−0.01
REG_QUAL	?	−7.56	−6.52	0.13	−0.18	0.41
RULE_OF_LAW	?	−3.88	4.04	0.92*	1.10**	−0.27
POL_FREEDOM	?	0.40*	0.19	−0.02**	−0.01	0.21**
HUMAN_RIGHTS	+	−0.87	−0.93*	0.07	0.06	−0.18***
OPENNESS	−	−21.58	−14.77	−6.74***	−0.21	−2.54**
EXPORTS	−	16.81	17.57	7.13***	0.65	1.60
MORTALITY	+	1.53	3.78	−0.04	−0.15*	−0.01
INFANT_MORT	+	−6.72	−6.96	0.10	0.24*	−0.02
LIFE_EXP	+	5.33	9.68	−0.09	0.33*	0.20***
PRIV_CAP	−	−5.91	−12.25	−3.38	−4.62***	−10.35***
BANK_CRED	−	−0.62	0.42	0.35	0.07	−0.65***
CRED_RATING	−	1.17	0.97	0.08	−0.22***	−0.23**
SAVINGS	?	−93.46***	−68.05**	−10.77**	−6.61***	−6.37***
POPULATION	−	−91.31	−98.90	15.15***	−5.75**	69.21
GDP	+	98.20	81.63	−0.83	8.49***	−50.23
GDP_GROWTH	+	−4.39	5.80	1.74	−1.84	3.15
GDP_PER_CAP	?	−97.09	105.47	15.13***	1.37	68.99
INFLATION	−	8.00	−2.50	−0.19	0.64	−0.18
LITERACY	?	−0.55	−6.42	0.03	−0.15**	−0.03
MILITARY_EXP	−	−5.14	−5.85	−0.17**	0.69***	0.07
US_MIL_AID	−	0.08	0.05	−0.01	0.00	0.03**
Intercept	+			−9.83		167.03***
Country Fixed Effects		Yes	Yes	No	Yes	No
Year Fixed Effects		Yes	Yes	Yes	Yes	Yes
n		93	93	96	258	179
Number of Countries		15	15	27	42	32
OLS R^2		59.4%	62.6%	63.0%	78.6%	73.8%

(continued on next page)

TABLE 8 (continued)

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.

All models use Generalized Least Squares (GLS), which corrects for panel-specific AR1 autocorrelation within countries, as well as heteroscedasticity. Coefficients on country and year indicator variables are not reported for brevity.

See Appendix A for definitions of remaining control variables.

Variable Definitions:

WB_LOANS = natural log of the $U.S. value of loan commitments made by the World Bank's International Bank for Reconstruction and Development (IBRD) to a country in year *t;*

CORRUPT = −1 multiplied by the control of corruption index in year *t* from Kaufmann et al. (2009). We classify this variable into high and low partitions based on the median value of 0.0 for all countries included in Kaufmann et al. (2009);

OPACITY = a measure of accounting quality as calculated in Bhattacharya et al. (2003) and is a composite of three measures of accounting quality: (1) earnings aggressiveness, (2) loss avoidance, and (3) earnings smoothing;

EARN_MGMT = a measure of earnings management following Leuz et al. (2003) and is a composite of four measures of earnings quality: (1) smoothing of reported operating earnings using accruals, (2) smoothing and the correlation between changes in accruals and operating cash flows, (3) the magnitude of accruals, and (4) small loss avoidance;

IFRS_DIFF = the difference between International Accounting Standards (IAS) and a country's domestic accounting standards, as compiled by Bae et al. (2008). It measures the difference between IAS and domestic GAAP in 21 key accounting rules;

POST_IFRS = an indicator variable equal to 1 in the years in which IFRS use is mandatory for all publicly traded firms in a country, and 0 otherwise;

AUD_QUAL = average of four aspects of a country's audit profession development. Each aspect includes individual components of Auditor Education, Audit Standards, Auditor Independence, and Auditor Oversight in a country; see Appendix C for details on *AUD_QUAL;* and

INTERACTION = in all model, indicates the interaction between *HIGH_CORRUPT* and our five respective test variables (for example, *HIGH_CORRUPT* ∗ *OPACITY*).

All of our results in Table 8 are robust to this except for one possible difference. The *POST_IFRS* analysis, while not significant at $p < 0.20$ in low-corruption countries, which is consistent with Table 8, is significant at only $p = 0.173$ (two-tailed) in high-corruption countries. Further, we note that our analysis of *AUD_QUAL* in high-corruption countries, represented by the combined coefficient, is now positive and significant, as predicted, at $p = 0.047$.

Fifth, we delete, in our main analyses, country-year observations where no loans were made to a country in a year to test the effect of accounting and audit quality on loan amounts for a sample of countries that actually receive loans. We do this because we believe accounting and audit quality are secondary determinants of loan amounts, whereas project need and construction ability are first-order determinants; see footnote 14 for a more detailed explanation. However, as a robustness test, we include these zero-value observations and perform a tobit regression while also clustering standard errors at the country level. All results for our *OPACITY* and *EARN_MGMT* variables are similar to those tabulated. The results for *POST_IFRS* are also robust, except in Table 4 where it is not significant. However, this is before partitioning the sample per H3 and H4. A further exception is that the p-value in high-corruption countries for *POST_IFRS* is only 0.154 (two-tailed). Our analyses using *IFRS_DIFF* and *AUD_QUAL* are generally not robust to the inclusion of zero loan amount observations. However, we note that *IFRS_DIFF* is the smallest accounting quality sample, and that the other three accounting quality measures are generally robust. Regarding the lack of robustness for our audit quality tests, we note that while audit quality does likely change from year to year to some extent, we find it difficult to believe that it changes so much that a negative change overwhelms the first-order determinants of World Bank loans. Therefore, we caution the reader to interpret this lack of a significant finding after including zero loan observations within this context.

Finally, we point out an important caveat regarding our tabulated results on the effect of IFRS use on World Bank loans given that, in four sensitivity tests above, results are not consistent with our tabulated analyses. Therefore, we caution the reader to consider this caveat when interpreting our post-IFRS results overall.

V. CONCLUSION

We investigate the role of accounting and auditing in the allocation of World Bank development aid lending. Access to development aid is crucial for developing countries to enable them to strengthen and modernize their infrastructures. Further, strong accounting and audit environments in a country are major infrastructure characteristics that can provide the World Bank with some confidence that development aid is being used for its intended purpose rather than being diverted for personal or political gain. We find that World Bank loan amounts are higher in countries where accounting and audit quality are stronger. However, we find this to be the case only in countries where there is low geo-political alignment with the United States, indicating that the World Bank, which is heavily influenced by the U.S., does not fully consider accounting and audit quality when geo-political interests of the recipient country are aligned with those of the U.S. Finally, we find that accounting and audit quality matter with regard to Bank loans only in countries with relatively high levels of corruption. This suggests that in countries with low corruption, Bank lenders rely on the limited level of corruption rather than considering the accounting and audit quality levels in a country when making loan amount decisions.

Our study is of interest to accounting and audit researchers as we provide new evidence of the importance of accounting and audit quality to the World Bank, a global stakeholder for which this issue has not previously been investigated. We also contribute to the economics and public policy literatures on international development aid by showing that accounting/audit quality in a country does not appear to be a factor in lending decisions for countries that are more aligned politically with the United States, or in countries with low corruption. This is in contrast to overall Bank guidance stating that accounting and audit quality are important loan considerations. We also contribute to the growing literature that analyzes the demand for and consequences of differential accounting and audit quality that exists across countries.

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APPENDIX A

Variable Definitions

Dependent Variable:

WB_LOANS = the natural log of the \$U.S. value of loan commitments made by the World Bank's International Bank for Reconstruction and Development (IBRD) to a country in year *t*.

Test Variables:

OPACITY = a measure of accounting quality calculated following Bhattacharya et al. (2003) and is a composite of three measures of accounting quality: (1) earnings aggressiveness, (2) loss avoidance, and (3) earnings smoothing. (1) is calculated as the median value of firm-level accruals scaled by total assets. Accruals are measured as net income before extraordinary items less cash flows from operations. (2) is calculated as the number of firms in a country-year group that report net income scaled by lagged total assets between 0 and 1 percent, minus the number of firms that report earnings scaled by lagged assets between −1 and 0, scaled by their sum. (3) is calculated as the correlation between the change in accruals and the change in operating cash flows from year *t*–1 to year *t* (both scaled by lagged total assets) for all firms in a country-year group. We then rank each of the above three country-year measures separately, into decile ranks. Finally, we average the three decile ranks for each country-year to form the final measure of *OPACITY*, which can range from 1 to 10 for each country-year observation. Firm-level accounting data were taken from Compustat Global.

EARN_MGMT = a measure of accounting quality following Leuz et al. (2003) and is a composite of four measures of accounting quality: (1) smoothing of reported operating earnings using accruals, (2) smoothing and the correlation between changes in accruals and operating cash flows, (3) the magnitude of accruals, and (4) small loss avoidance. (1) is calculated as the country-year median ratio of the firm-level standard deviation of operating earnings scaled by the firm-level standard deviation of cash flows from operations, (2) is calculated as the country-year contemporaneous correlation between changes in accounting accruals and changes in operating cash flows, (3) is calculated as the country-year median of the absolute value of firms' accruals scaled by the absolute value of a firm's cash flows from operations, and (4) is calculated as the country-year ratio of small reported profits to small reported losses, where losses and profits are considered small if the amount scaled by total assets is between 0 and 1 for profits and −1 and 0 for losses. Leuz et al. (2003) calculate all components at the country level. We augment the Leuz et al. (2003) measures by calculating each component at the country-year level so as to obtain a measure of earnings management for each country in each year. This allows us to implement a "changes" specification. We then rank each of the above four country-year measures separately, into decile ranks. Finally, we average the four decile ranks for each country-year to form the final measure of *EARN_MGMT*, which can range from 1 to 10 for each country-year observation. Firm-level accounting data were taken from Compustat Global.

IFRS_DIFF = the difference between International Accounting Standards (IAS) and a country's domestic accounting standards (DS) as compiled by Bae et al. (2008). *IFRS_DIFF* measures the difference between IAS and domestic GAAP in 21 key accounting rules. The differences are based on a survey of partners in large accountancy organizations from more than 60 countries benchmarking local GAAP rules to IAS rules in place as of December 31, 2001. A higher score indicates greater differences between local accounting standards and IAS, which we interpret as an indicator of lower accounting quality, consistent with Bae et al. (2008). For countries that have adopted IFRS, this variable is set equal to 0 (which is an augmentation to Bae et al. 2008).

POST_IFRS = an indicator variable equal to 1 in the years in which IFRS use is mandatory for all publicly traded firms in a country, and 0 otherwise (Deloitte 2003, 2005, 2006, 2007, 2009).

AUD_QUAL = the average of four aspects of a country's audit profession development, as in Michas (2011). Each aspect includes individual components of Auditor Education, Auditing Standards, Auditor Independence, and Auditor Oversight in a country. See Appendix C for details. This variable is used by Michas (2011) in his investigation of audit profession development in emerging market countries.

UN_VOTING = the alignment, in percentage terms, of a country's voting in the U.N. General Assembly with U.S. voting (U.S. Department of State 2014a). These data can be found at http://www.state.gov/p/io/rls/rpt/. We classify this variable into high and low partitions, where countries are classified as high-alignment countries when their votes align with the U.S. at the rate of 50 percent or more in a year. We use this variable in our Table 5 analyses. This variable is also used by Andersen et al. (2006) in their investigation of World Bank International Development Assistance (IDA) lending, and by Kilby (2009) in his investigation of the conditionality of World Bank loan disbursements.

UN_SC_MEMBER = an indicator variable equal to 1 in years in which a country has been elected as a temporary member to the United Nations Security Council, and 0 otherwise. We use this variable in our Table 6 analyses. This variable is used by Dreher et al. (2009) in their investigation of whether membership in the U.N. Security Council affects the number of World Bank projects started in a country in a year.

DISTANCE = the geographical distance, in miles, from a country's capital city to Washington, DC. We split this measure into high and low partitions based on the median value for all countries in our largest sample. We use this variable in our Table 7 analyses. This variable is used by Neumayer (2003b) in his investigation of regional multilateral development banks and United Nations Agencies.

CORRUPT = −1 multiplied by the control of corruption index in year *t* from Kaufmann et al. (2009); the index captures the "perceptions of the extent to which public power is exercised for private gain, including both petty and grand forms of corruption, as well as 'capture' of the state by elites and private interests." This variable ranges from a theoretical minimum of −2.50 to a maximum of 2.50. We multiply by −1 so that higher values indicate a higher level of corruption in a country-year. We classify this variable into high and low partitions based on the median value of 0.0 for all countries included in Kaufmann et al. (2009) and use this variable in our Table 8 analyses. The continuous version of this variable is also used as a control in all other analyses. This variable is also used by Alesina and Weder (2002) in their investigation of whether corrupt governments receive less World Bank International Development Assistance (IDA) lending, and by Neumayer (2003a) in his investigation of development assistance.

Control Variables:

IFRS_ADOPTER = an indicator variable equal to 1 if a country mandates IFRS use for all publicly traded companies at some point during the sample period, and 0 otherwise (Deloitte 2003, 2005, 2006, 2007, 2009).

DEMOC = the democracy rating issued by Marshall, Gurr, and Jaggers (2014) in year *t*. The variable ranges from 0 to 10, with higher values being equated with stronger levels of democracy in a country. These data can be found at http://www.systemicpeace.org/inscr/p4v2013.xls. This variable is also used by Alesina and Weder (2002) and Neumayer (2003a).

REG_QUAL = the regulatory quality index from Kaufmann et al. (2009); the index captures "the perceptions of the ability of the government to formulate and implement sound policies and regulations that permit and promote private sector development." This variable ranges from a theoretical minimum of −2.50 to a maximum of 2.50. The median value over all countries in Kaufmann et al. (2009) is 0.0. This variable is also used by Andersen et al. (2006) and Neumayer (2003a).

RULE_OF_LAW = the rule of law index from Kaufmann et al. (2009); the index captures "the perceptions of the extent to which agents have confidence in and abide by the rules of society, and in particular the quality of contract enforcement, property rights, the police, and the courts, as well as the likelihood of crime and violence." This variable ranges from a theoretical minimum of −2.50 to a maximum of 2.50. The median value over all countries in Kaufmann et al. (2009) is 0.0. This variable is also used by Andersen et al. (2006) and Neumayer (2003a).

POL_FREEDOM = a combined freedom index calculated as the sum of the political rights and civil liberties indices compiled by Freedom House (2011). The combined index was multiplied by −1 so that it ranges from −2 (best) to −14 (worst). These data can be found at http://www.freedomhouse.org/report/freedom-world-aggregate-and-subcategory-scores. This variable is also used by Andersen et al. (2006) and Neumayer (2003b, 2003c).

HUMAN_RIGHTS = a combined human rights index calculated by adding the two Political Terror Scales from Gibney, Cornett, Wood, and Haschke (2013). These data can be found at http://www.politicalterrorscale.org. This variable is also used by Andersen et al. (2006) and Neumayer (2003a, 2003b).

OPENNESS = the natural log of the value of imports into plus exports from a country in year *t*. This variable is also used by Andersen et al. (2006).

EXPORTS = the natural log of the value of exports of goods and services from a country in year *t*. This variable is also used by Neumayer (2003a, 2003b).

MORTALITY = the mortality (death) rate in percentage terms of those under five years of age in a country in year *t*. We include this to control for the overall level of medical quality in a country and the corresponding need for external financing to improve this. The U.S. Centers for Disease Control (2011) indicate this variable is an effective measure of medical quality in a country.

INFANT_MORT = the mortality (death) rate for every 1,000 births in a country in year *t*. We include this to control for the overall level of medical quality in a country and the corresponding need for external financing to improve this. The U.S. Centers for Disease Control (2011) indicate this variable is an effective measure of medical quality in a country.

LIFE_EXP = the life expectancy at birth, in years, of the population in a country in year *t*. We include this to control for the overall level of societal development and medical quality in a country, which are main emphases of the World Bank (World Bank 2014f).

PRIV_CAP = the value of total private capital flows into a country in year *t*, scaled by GDP. We include this to control for other sources of capital flowing into a country that can be used in the private sector to improve a country's infrastructure.

BANK_CRED = the value of domestic credit provided by the banking sector in a country in year *t*, scaled by GDP. We include this to control for the amount of bank funding available within a country that can be used in the private sector to improve a country's infrastructure.

CRED_RATING = a measure of the depth of information available in a country affecting the scope, accessibility, and quality of credit through public or private credit registries. This variable ranges between a minimum of 0 to a maximum of 6. We include this to control for the overall perceptions of the ability of a country to repay its outstanding debts.

SAVINGS = the value of gross domestic savings in a country in year *t*, scaled by GDP. We include this to control for the amount of private savings that can be allocated to the private sector to improve a country's infrastructure.

POPULATION = the natural log of the total population in a country in year *t*. This variable is also used by Andersen et al. (2006), Dreher et al. (2009), and Neumayer (2003a, 2003b, 2003c).

GDP = the value of gross domestic product in a country in year *t*. This variable is log-transformed in all regression analyses. This variable is also used by Neumayer (2003b, 2003c).

GDP_GROWTH = the percentage change in *GDP* in a country from year *t*–1 to year *t*. We include this to control for changes in an economy that may affect a country's need for external financing.

GDP_PER_CAP = the value of gross domestic product in a country in year *t*, scaled by population. This variable is log-transformed in all regression analyses. This variable is also used by Andersen et al. (2006), Alesina and Weder (2002), and Neumayer (2003a, 2003b).

INFLATION = the inflation rate of consumer prices in a country in year *t*. We include this to control for the volatility level of a country's economy, which may affect the willingness of outside lenders to loan funds to a country.

LITERACY = the literacy rate, in percentage terms, of the adult population who are 15 years and older. We include this to control for the overall level of societal and educational development in a country, which are main emphases of the World Bank (World Bank 2014f).

MILITARY_EXP = the natural log of a country's total military expenditures in year *t*. This variable is also used by Andersen et al. (2006), Dreher et al. (2009), and Neumayer (2003a, 2003b, 2003c).

US_MIL_AID = the natural log of the amount of U.S. military aid provided to a country in year *t* (U.S. Department of State 2014b). These data were compiled from the U.S. Department of State website (for example, see: http://www.state.gov/t/pm/ppa/sat/c14560.htm). This variable is also used by Andersen et al. (2006), Dreher et al. (2009), and Neumayer (2003b).

All variables are taken from the World Bank's World Development Indicators (World Bank 2014a) database unless otherwise noted. All currency value variables are in $U.S.

APPENDIX B

Description of the International Bank for Reconstruction and Development (IBRD)

The International Bank for Reconstruction and Development (IBRD) "aims to reduce poverty in middle-income and creditworthy poorer countries by promoting sustainable development through loans, guarantees, risk management products, and analytical and advisory services" (World Bank 2012b). The IBRD is structured like a cooperative and is owned and operated for the benefit of its 188 member countries.

Specifically, the IBRD supports long-term human and social development needs that private creditors do not finance, preserves borrowers' financial strength by providing support in crisis periods, uses the leverage of financing to promote key policy and institutional reforms, creates a favorable investment climate in order to catalyze the provision of private capital, and provides financial support in areas that are critical to the well-being of poor people in all countries (World Bank 2012b).

The IBRD has had triple-A status with credit rating agencies since 1959 and, thus, borrows at attractive rates on the world's financial markets. The IBRD covers its operating expenses primarily out of its income from the return on its equity and from the small margin it makes on lending.

IBRD financial products and services include loans, contingent financing, credit enhancement, hedging products, catastrophe risk management, and client advisory services.

Middle-income and creditworthy lower-income countries may qualify for IBRD loans. The Bank classifies a country according to the wealth of its population: middle-income countries are defined as having a per capita income of between around US$1,000 and US$10,000, low-income countries with a per capita income of less than $1,000 usually do not qualify for IBRD loans unless they are creditworthy (World Bank 2012b). These low-income countries, however, are eligible to receive low or no interest loans and grants from another World Bank organization, the International Development Association (IDA).

The IBRD General Conditions for Loans require that the borrower country maintain a financial management system and prepare financial statements in accordance with consistently applied accounting standards acceptable to the Bank, both in a manner adequate to reflect the operations, resources, and expenditures related to the project for which the loan is extended (IBRD 2010).[21] In addition, the borrower must have the project financial statements periodically audited by independent auditors acceptable to the Bank, in accordance with consistently applied auditing standards acceptable to the Bank (IBRD 2010).[22]

APPENDIX C

Measurement of Audit Profession Development and Description of Data Sources[23]

The 13 components are coded by answering the following 13 questions (data source is in parentheses).[24]

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[21] Accounting standards acceptable to the Bank include International Public Sector Accounting Standards and International Financial Reporting Standards/International Accounting Standards. The Bank may accept national accounting standards that it considers to be equivalent to international standards (World Bank 2013, OP 10.00, 6).

[22] Auditing standards acceptable to the Bank include Auditing Standards issued by the International Organization of Supreme Audit Institutions and International Standards on Auditing issued by the International Federation of Accountants. The Bank may accept national auditing standards that it considers to be equivalent to international standards (World Bank 2013, OP 10.00, 6).

[23] Appendix C is reproduced from Appendix B in Michas (2011). See Michas (2011) for more detailed descriptions of the World Bank's ROSC reports and the International Federation of Accountants (IFAC) Assessments of the Auditing Regulatory Standard Setting Framework.

[24] Unless otherwise noted, the questions are coded 1 when the answer is yes, and 0 otherwise. (ROSC) after a component indicates the data were taken from the World Bank's Reports on the Observance of Standards and Codes. (AARSSF) after a component indicates the data were taken from IFAC's Assessments of the Auditing Regulatory Standard Setting Framework. For question #8, the AARSSF reports were used to collect data for Bulgaria, Colombia, India, Indonesia, Jordan, Mauritius, Mexico, Romania, and Turkey. For question #9, the AARSSF reports were used to collect data for Bulgaria, China, Colombia, Egypt, Morocco, Peru, Romania, and Turkey. For question #12, the AARSSF reports were used to collect data for all countries except Egypt.

(A) Auditor Education:

1. Are universities' accounting education curriculum standards the same for all universities within a country (ROSC)?
2. Are auditors required to perform on a professional examination before being licensed to practice as an auditor (ROSC)?
3. Are accountants required to gain professional experience before being licensed as an auditor (ROSC)?
4. Are auditors required to fulfill continuing education requirements on an annual basis (ROSC)?

(B) Auditing Standards:

5. To what extent are the country's auditing standards consistent with International Standards on Auditing? This variable is coded 0.00 if there is low consistency, 0.33 if medium, 0.67 if high, and 1.00 if they are exactly the same except for very minor differences (ROSC).

(C) Auditor Independence:

6. Are auditors in the country prohibited from both preparing and auditing a client's financial statements (ROSC)?
7. What is the level of liability faced by auditors in the country? This variable is coded 0.00 if liability is nonexistent, 0.33 if low, 0.67 if mid-level, and 1.00 if high (ROSC).
8. Are companies' audit committees responsible for appointing listed companies' external auditors (ROSC and AARSSF)?
9. Is auditor rotation required for external auditors of listed companies (ROSC and AARSSF)?
10. Has the audit profession adopted the ethics code of the International Federation of Accountants (ROSC)?

(D) Auditor Oversight:

11. Are auditors required to register with or be licensed by a central governing organization, either public or private (ROSC)?
12. What type of auditor practice reviews are mandatory within the country? This variable is coded 0.00 if none are required, 0.50 if a government body or a peer auditor conducts the review, and 1.00 if an independent, professional audit organization (similar to the American Institute of Certified Public Accountants [AICPA], for example) conducts the review (ROSC and AARSSF).
13. Does an organization within the country consistently issue published audit implementation guidelines (ROSC)?

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Impaired Judgment: The Effects of Asset Impairment Reversibility and Cognitive Dissonance on Future Investment

Kristina Rennekamp
Cornell University

Kathy K. Rupar
University of Florida

Nicholas Seybert
University of Maryland College Park

ABSTRACT: This paper examines how the reversibility of the accounting effect of asset impairments affects managers' investment decisions. We conduct two experiments in which participants act as CEO of a multi-division electronics company that suffers a large asset impairment at one of the divisions. Drawing on prior psychology research involving cognitive dissonance and decision reversibility, we predict and find that managers who are responsible for the decision to record the asset impairment invest more in the impaired division when the accounting effect of the impairment is reversible than when it is irreversible. This is consistent with the idea that reversible accounting effects encourage behavioral attempts to alter the cash flow outcome, while irreversible accounting effects encourage belief revision to rationalize the cash flow outcome. Also in line with cognitive dissonance theory, we show that managers who are not responsible for the decision to impair the asset, or managers who are given the opportunity to deny responsibility for the asset impairment, do not differ in their investment in the impaired division, regardless of impairment reversibility.

Keywords: *impairment; reversal; investment; decision making; cognitive dissonance.*

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I. INTRODUCTION

The accounting treatment for asset impairment reversals represents one important and controversial difference between International Financial Reporting Standards (IFRS) and U.S. Generally Accepted Accounting Principles (GAAP). Under GAAP, asset impairment cannot be reversed in the future, even when expected future cash flows exceed their impaired book value. Under IFRS, however, reversal of impairment is required in such circumstances. Recent research demonstrates that the use of alternative accounting methods can affect asset disposal and project choices (e.g., Jackson 2008; Jackson, Rodgers, and Tuttle 2010; Seybert 2010). Related research demonstrates that managers sometimes change investment decisions to meet reporting goals (e.g., Bhojraj and Libby 2005; Graham, Harvey, and Rajgopal 2005). These studies examine effects of reporting effects on investment decisions. They also demonstrate that the findings can be explained by opportunistic behavior aimed at portraying positive performance in current financial reporting disclosures.[1] Unlike these prior studies, we hold underlying firm economics constant and suggest that allowing or disallowing future impairment reversal will affect managers' current investment decisions as a result of the cognitive dissonance induced by a large asset impairment. Because asset impairment and investment decisions involve considerable discretion (Nelson, Elliott, and Tarpley 2002; Hilton and O'Brien 2009), there is also ample room for unintentional biases to affect those decisions.

Our theory draws from prior psychology research showing that individuals experience an internal conflict when negative events threaten their positive self-perceptions. This internal conflict is also known as cognitive dissonance, which individuals alleviate either via changes in behavior or via rationalization (Festinger 1957; Gilbert, Pinel, Wilson, Blumberg, and Wheatley 1998). Managers may experience cognitive dissonance when recording an asset impairment threatens their confidence in their abilities. When they know that reporting effects can be altered via changes in behavior, as is the case when the accounting for impairments is reversible, there is less need to alter their beliefs in order to reduce cognitive dissonance. In contrast, when reporting effects cannot be changed, such as when the accounting for impairments cannot be reversed, managers are more likely to try to reduce dissonance by rationalizing the poor performance of the asset in a manner that allows them to maintain a positive self-image.

In Experiment 1, we contrast the behavior of a manager responsible for the decision to record an asset impairment with that of a manager not responsible for the decision to record an asset impairment. Prior research suggests that cognitive dissonance is induced when individuals feel responsible for negative outcomes that conflict with their self-perceptions (Brehm and Cohen 1962; Linder, Cooper, and Jones 1967; Collins and Hoyt 1972). In an asset impairment setting, the act of recording the impairment could induce dissonance if the manager *feels* responsible for the negative performance and/or *perceives* that external stakeholders view her as responsible for the negative reporting effect. Both possibilities conflict with a manager's positive beliefs about her abilities as a manager. We predict and find that participants assuming the role of a CEO who *is* responsible for the decision to record an asset impairment subsequently invest more (less) in the impaired division when the impairment is reversible (irreversible), as compared to participants assuming the role of a CEO who *is not* responsible for the decision to record the asset impairment.[2]

In Experiment 2, we provide further evidence that cognitive dissonance is the psychological mechanism underlying our results. We investigate whether providing managers with an alternative mode of dissonance reduction, by allowing them to deny responsibility for the impaired division's poor performance, decreases the impact of cognitive dissonance on their subsequent investment decisions. In this setting, which we adapt from Gosling, Denizeau, and Oberle (2006), we predict and find that managers given the opportunity to deny responsibility for the impaired division's poor performance respond similarly to managers in the not-responsible conditions in our first experiment. That is, they do not exhibit signs of cognitive dissonance. In contrast, managers not given the opportunity to deny responsibility exhibit biases consistent with the responsible conditions in our first experiment. That is, they allocate more funds to the impaired division when the impairment is reversible than when it is irreversible.

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[1] Jackson (2008) is an exception in that he examines the effect of a past depreciation method on perceptions of retrospective utility provided by the asset.

[2] We use the "not-responsible" condition as a baseline in which we expect no difference as a result of the reversibility manipulation, since a lack of responsibility does not induce dissonant beliefs in the manager.

Our paper makes three contributions. First, we extend prior asset impairment research by showing that firms' future investment decisions with respect to impaired assets can be affected not only by real economic differences between firms, but also by cosmetic reporting differences in the form of the reversibility or irreversibility of impairments for accounting purposes. This finding should be of interest to regulators as it suggests an unintended consequence of the reversibility of the accounting effect of impairments. Second, our findings inform managers and investors about potential differences in impaired asset investments, depending on whether managers are initially responsible for the impairment decision. We confirm that in practice, it is relatively common for the same individual to be responsible for both an impairment decision as well as a subsequent investment decision related to the impaired asset, because both tasks require an understanding of the expected future cash flows associated with the asset. Third, we provide evidence for the process underlying this unintended effect. In the accounting literature, the impact of cognitive dissonance reduction on managers' beliefs has been previously applied to a setting involving an ethical dilemma (Brown 2014; Brown, Rennekamp, Seybert, and Zhu 2014). We expand the literature on cognitive dissonance in accounting decisions by identifying its cash flow effects in a setting with no ethical dilemma. Further, our second experiment suggests measures that firms may take to reduce the impact of cognitive dissonance reduction on managers' investment decisions. Our results also suggest potentially broader implications for decisions other than asset impairment. In the "Conclusion" section, we discuss the implications of our results for other accounting and operational decisions, both of which could have real economic consequences.

Next, Section II provides background information and develops our hypotheses. Sections III and IV discuss our experiments, and Section V concludes.

II. BACKGROUND AND HYPOTHESES

Accounting for Asset Impairment

Both U.S. Generally Accepted Accounting Principles (GAAP) and International Financial Reporting Standards (IFRS) require that a company write down an asset's value and record an impairment loss when the asset is determined to be impaired. However, the two sets of standards differ in how a company should account for any subsequent reversals in asset impairment. Under GAAP, Accounting Standards Codification (ACS) 360-10-35-20 states that "restoration of a previously recognized impairment loss is prohibited" (Financial Accounting Standards Board [FASB] 2011), while International Accounting Standard (IAS) 36 provides guidance on how to record any post-impairment increases in asset value if a change in the asset's value suggests that the impairment amount has decreased or no longer exists (International Accounting Standards Board [IASB] 2012).

Prior literature focuses on how differences in economic incentives and actual asset values between firms affect the potential costs and benefits of impairment reversals. On the one hand, upward asset revaluations are positively associated with future changes in operating performance (Aboody, Barth, and Kasznik 1999) and increases in value relevance (Aharony, Barniv, and Falk

2010), suggesting that firms may use impairment reversals to convey useful information about asset values. On the other hand, research shows that managers may use impairment reversals to write up asset values in order to avoid trading suspension or delisting (Chen, Wang, and Zhao 2009). Further, Aboody et al. (1999) show that the correlation between impairment reversals and operating performance lessens for firms facing high debt-to-equity ratios, suggesting that firms may instead use impairment reversals opportunistically rather than to convey useful information. Our study contributes to the literature on the costs and benefits of allowing reversals of asset impairments by showing that, holding underlying firm incentives and asset values constant, simply having the choice to record an impairment reversal will alter managers' operating decisions.

Cognitive Dissonance Reduction

One of the more common findings in the psychology literature is that individuals are adept at generating positive feelings about themselves and their situations (Gilbert et al. 1998). When their positive feelings are threatened by a negative event, individuals rationalize the event by revising their beliefs in order to maintain those feelings. One widely studied psychological mechanism that promotes such rationalization is the need to reduce cognitive dissonance. Cognitive dissonance refers to the discomfort that individuals feel when they hold multiple simultaneous cognitions or beliefs that are in conflict (Festinger 1957). For instance, individuals experience cognitive dissonance when their actions do not align with their preferred beliefs about themselves. Cognitive dissonance *reduction* refers to individuals' tendency to reduce these feelings of discomfort by altering their beliefs. In an accounting setting, Brown (2014) relies on theories of cognitive dissonance reduction to predict that engaging in earnings management will alter managers' beliefs about the acceptability of earnings management. He argues that choosing to manage earnings creates cognitive dissonance in individuals because it conflicts with their desire to view themselves as honest. To reduce this dissonance, managers change their beliefs and view mild forms of earnings management as more acceptable.

A similar effect may occur in asset impairment contexts, in which a manager decides to write down an asset's value, which is a negative event suggesting unsuccessful management of the asset, yet wishes to maintain a positive perception about her ability to optimize the asset's performance. As displayed in Figure 1, we expect that managers will experience dissonance when their decision to write down an asset induces perceived responsibility for poor performance and does not align with their belief that they are capable overseers of that asset (see Link 2 in Figure 1).[3] This dissonance can be induced in two ways: (1) Responsibility for taking an impairment induces a feeling of responsibility for the poor performance of the impaired division, thus threatening the positive self-perceptions of the manager, and/or (2) Responsibility for taking an impairment induces a belief that external stakeholders perceive the manager as responsible for the poor performance of the impaired asset, similarly threatening the positive self-perceptions of the manager.[4] Our primary research question is whether the way managers deal with this dissonance depends on accounting rules regarding the reversibility of the impairment.

Decision Reversibility

Cognitive dissonance reduction allows individuals to maintain positive feelings about themselves when their decisions lead to negative outcomes. Research by Frey, Kumpf, Irle, and

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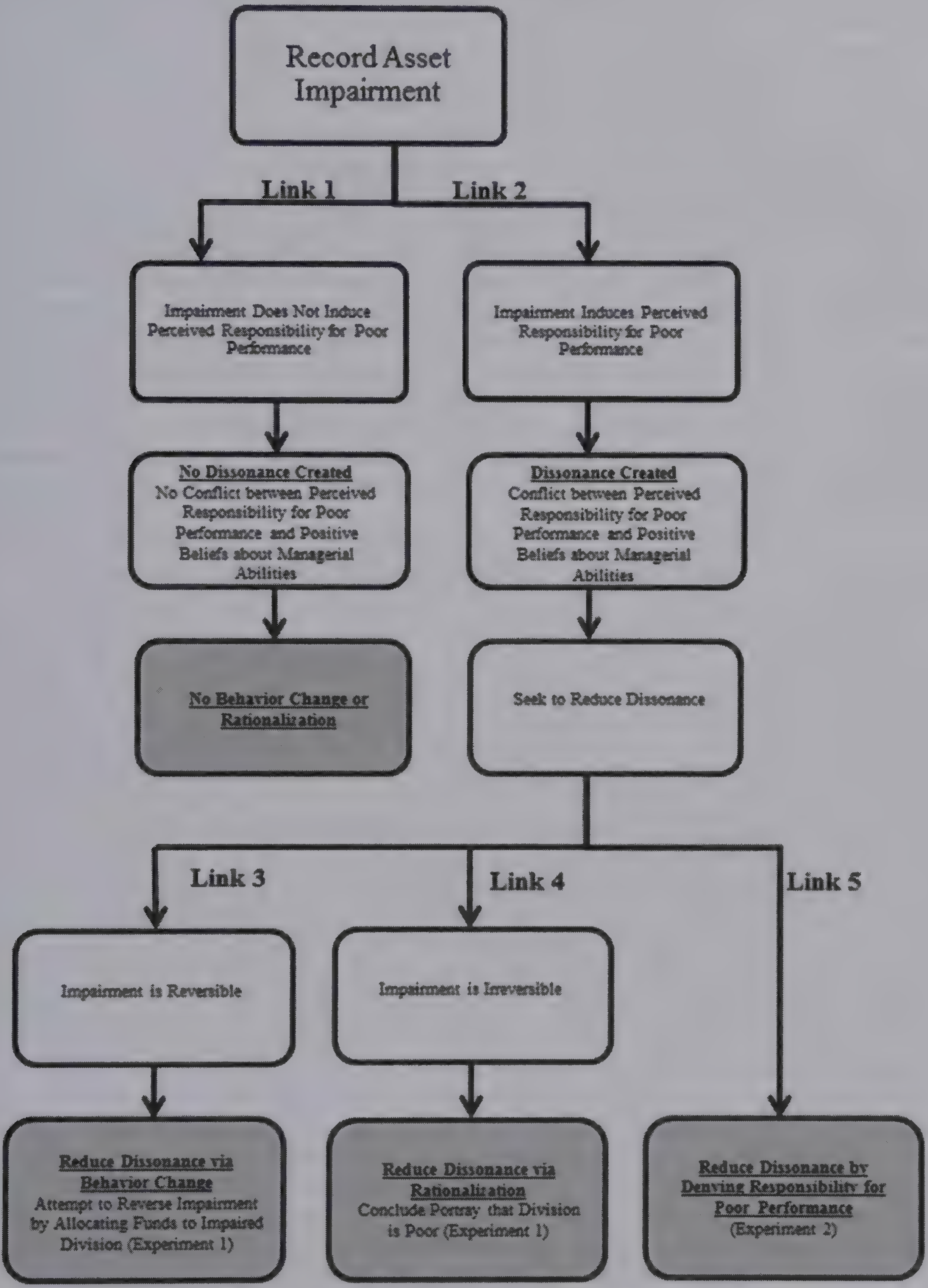
[3] Figure 1 also shows that when managers are not responsible for the decision to impair the asset, no dissonance is created and, therefore, there is no need for either behavior change or rationalization (see Link 1 in Figure 1).

[4] Our experiments do not distinguish between these two possibilities and, in fact, both are likely to play a role in inducing cognitive dissonance in managers who are responsible for the decision to impair an asset.

FIGURE 1

Theoretical Framework


Record Asset Impairment
Link 1
Link 2
Impairment Does Not Induce Perceived Responsibility for Poor Performance
Impairment Induces Perceived Responsibility for Poor Performance
No Dissonance Created
No Conflict between Perceived Responsibility for Poor Performance and Positive Beliefs about Managerial Abilities
Dissonance Created
Conflict between Perceived Responsibility for Poor Performance and Positive Beliefs about Managerial Abilities
No Behavior Change or Rationalization
Seek to Reduce Dissonance
Link 3
Link 4
Link 5
Impairment is Reversible
Impairment is Irreversible
Reduce Dissonance via Behavior Change
Attempt to Reverse Impairment by Allocating Funds to Impaired Division (Experiment 1)
Reduce Dissonance via Rationalization
Conclude Portray that Division is Poor (Experiment 1)
Reduce Dissonance by Denying Responsibility for Poor Performance
(Experiment 2)

(continued on next page)

FIGURE 1 (continued)

This figure presents an outline of how we reach our predictions in experiments 1 and 2. More specifically, it shows when cognitive dissonance is (or is not) created, based on managers' responsibility (or lack of responsibility) for an impairment decision and/or performance, as well as how cognitive dissonance can be alleviated through either changes in behavior or rationalization.

Gniech (1984) and Gilbert and Ebert (2002) suggests that decision reversibility hinders this process, while knowledge about the decision's irreversibility promotes it. Specifically, Frey et al. (1984) show that participants choosing alternatives that they could reverse in the future reported significantly lower levels of satisfaction with the chosen alternative than did participants choosing alternatives that they could not reverse. Moreover, these same participants reported significantly higher levels of satisfaction with the non-chosen alternative than did participants choosing alternatives that they could not reverse. Similarly, Gilbert and Ebert (2002) find that photography students who had an option to change their mind regarding which of two prints to keep reported lower levels of satisfaction with their chosen print than students who did not have the option.

It appears that participants in both studies were unable to accept and, therefore, be satisfied with their chosen alternatives given knowledge that other choices were still available. Conversely, participants informed that their choices were final provided satisfaction ratings consistent with the process of dissonance reduction taking effect. That is, unencumbered by thoughts of alternative choices, participants making a binding choice were forced to rationalize their decision and conclude that they made a satisfactory selection.

The above studies suggest that managers recording an asset impairment for which the accounting effect is reversible will have difficulty accepting that the asset has permanently lost value and, thus, will attempt to reduce dissonance by altering the impairment outcome. In the context of impairment and investment, the allocation of additional funds to turn the division around is the clearest path to achieving the reversal. Consequently, managers will allocate funds to the impaired division in hopes of reversing the outcome (see Link 3 in Figure 1). In contrast, when the accounting effect is irreversible, managers are left with little choice but to rationalize the impairment outcome by concluding that the division would have suffered setbacks regardless of their own managerial ability. If the manager can deflect blame onto the division by concluding and/or portraying that it was simply a lost cause, then the manager can maintain positive illusions of her own ability. Consequently, managers will be less willing to allocate funds to the impaired division, in line with the belief that the division's performance cannot improve (see Link 4 in Figure 1).

It is important to note that prior psychology research finds that behavior change overwhelms rationalization when decisions are reversible, and rationalization overwhelms behavior change when decisions are irreversible (Gilbert and Ebert 2002). Our context is slightly different because, although an irreversible impairment should force managers to accept that the division has lost value and, thus, the impairment was necessary, we explicitly ask participants to make an investment decision that should reflect this belief. Hence, we extend the prior psychology studies by examining whether rationalization can subsequently alter behavior in the corresponding direction.

While negative decision outcomes often produce cognitive dissonance, individuals are less likely to experience this effect and, therefore, experience less need to reduce it when they feel less responsibility for the negative outcome. Prior studies have found that when participants do not perceive personal responsibility for a decision, they experience significantly lower levels of dissonance (Brehm and Cohen 1962; Linder et al. 1967). Collins and Hoyt (1972) find that "a subject must feel personally responsible for an act which appears to have important consequences" in order to produce dissonance. Similarly, individuals experience less guilt and regret (Tykocinski and Steinberg 2005) and are more likely to maintain positive emotions (Siemer, Mauss, and Gross

2007) when they perceive that they have less responsibility for a negative outcome. Of particular relevance to our irreversible impairment conditions, Tykocinski and Steinberg (2005) show that those who feel that they have *greater* control over a failed attempt to achieve a desired goal use more retroactive pessimism as a defense mechanism—thus convincing themselves that failure was inevitable all along. In summary, the prior literature suggests that one way to provide evidence of dissonance reduction is to show that its predicted effects occur when individuals feel responsible for a decision, but that these effects are reduced or eliminated when they do not feel responsible for a decision.

Based on the above discussion, we hypothesize that:

> **H1:** Managers responsible for the decision to impair an asset will invest more (less) in the impaired division when the accounting effect of the impairment is reversible (irreversible) compared to managers who are not responsible for the impairment decision.

We expect that impairment decisions will affect future investment decisions, in part, because it is relatively common for the same manager to have responsibility for both decisions within an organization. Impairment decisions require the estimation of expected future cash flows associated with an asset, in order to determine whether the ability of the asset to generate cash flows has deteriorated. Likewise, allocating future investment dollars to an impaired asset requires understanding how future investment might affect the expected future cash flows of the asset. Because both decisions require a fundamental understanding of the expected pattern of cash flows associated with an asset, both will sometimes be the responsibility of the same individual within a firm.

III. EXPERIMENT 1

Participants

Participants are 66 graduate and undergraduate students and alumni from the University of Maryland and Yale University business schools. They participate through an online laboratory platform called the Smith eLab, where a variety of studies are openly listed to these participant groups. They are compensated $0.50 for completing the "ten-minute" study, which approximates the average hourly rate for experiments using the Smith eLab (see Read, Frederick, and Scholton [2013] for an example of comparable compensation using the Yale eLab). Twenty-seven percent of participants are undergraduate alumni, 18 percent are current graduate students or graduate alumni, and the remaining 55 percent are current undergraduate students. Forty-five percent of participants have full-time work experience, with average work experience of 2.4 years. They have taken an average of 2.5 accounting and finance courses, and 18 percent have taken five or more accounting and finance courses. Ninety percent of participants are native English speakers. Given that we investigate a psychological phenomenon that we do not *a priori* expect to differentially affect managers and non-managers, we believe that we have properly matched participants to the goals of our experiment (Libby, Bloomfield, and Nelson 2002).

Design

Our experiment utilizes a 2 × 2 between-subjects design and manipulates the reversibility of the accounting effect of an asset impairment (reversible, irreversible), and whether the manager is responsible for the decision to impair the asset (responsible, not responsible). We hold constant underlying firm characteristics, the performance of the four divisions within the firm, and the dollar amount of the impairment. We manipulate the reversibility of the accounting effect of the asset impairment by asking half the participants to assume that if future cash flows improve, then the

FIGURE 2

Responsibility for Impairment Decision Manipulation

Prior Years	RESPONSIBLE Became CEO Last Year		NOT RESPONSIBLE Became CEO This Year	Task:
	Participant Informed:		Participant Informed:	
Firm Inception; Equal investment in four divisions determined by previous CEO	"Because accounting standards require you to "impair" assets….you made the decision to impair the values by approximately 50%..."	vs.	"Because accounting standards required your predecessor to "impair" assets….he made the decision to impair the values by approximately 50%..."	"Your task as current CEO is to decide how much of the total budget to allocate to each division"

prior accounting for the impairment is reversible, while asking the remaining participants to assume that *even if* future cash flows improve, the prior accounting for the impairment is *irreversible*.[5]

As depicted in Figure 2, we manipulate impairment decision responsibility by having participants act as current CEO and allocate the impairment to three different asset classes within the division, or informing them that their predecessor was responsible for the impairment decision and made an equal allocation across the three asset classes within the impaired division.[6,7] This manipulation is consistent with the typical treatments used in prior responsibility studies, such as Seybert (2010). We hold constant that neither CEO was in charge of initially investing in the now-impaired division, which has been in place for several years. We do so by informing participants that this is either their second (responsible for impairment decision condition) or first (not responsible for impairment decision condition) year as a CEO of a large company. In the subsequent year of firm operations, participants must allocate $800 million of research and development (R&D) funding across the firm's four divisions. The amount allocated to the impaired division is the dependent variable of interest.[8]

We did not provide performance-based incentives because our predictions are driven by behavioral theory rather than economic theory. As Libby et al. (2002) indicate, past experiments suggest that incentive compensation does not eliminate behavioral biases because they are driven by limitations in cognitive abilities rather than a lack of effort. Thus, we compensated participants with a flat fee. In the "Conclusion" section, we note that this limits the degree to which our findings can be generalized to settings in which individuals face performance-based incentives.

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[5] To ensure that our participants understood the distinction between an accounting effect of impairment and a true change in the future cash flows resulting from the asset, we also provided participants with the following example: "For example, if future performance implies that a value of $300 million is more appropriate than the $265 million currently recorded, the $35 million dollar increase in value can [cannot] be recorded in the balance sheet and will [will not] be recorded as a $35 million dollar gain in reported net income." If participants do not understand this distinction and instead believe that "reversible" relates to a better-performing asset than "irreversible," then we would expect to obtain a main effect of reversibility rather than the interaction between responsibility and reversibility we predict in H1.

[6] Participants asked to make the allocation decision assigned percentages to each of the three asset classes that did not statistically differ from 33 percent (i.e., an equal allocation).

[7] While this allocation decision serves to reinforce the responsibility manipulation in our study, we note that it also aligns with the discretion managers have under U.S. GAAP and IFRS surrounding the impairment decision. For example, IAS 36 requires that a Cash-Generating Unit (CGU) should be the smallest asset unit for which cash flows are generally separable, but that management must exercise judgment when identifying this lowest level of aggregation. Therefore, allocating percentage of total impairment to CGUs is similar to providing the manager some maneuverability in the individual cash flow forecasts for each product line.

[8] While our analyses are conducted using the raw dollar amount that participants choose to allocate, all inferences and results discussed in Sections III and IV are unchanged if we instead use the proportion of the total $800 available that is allocated to the impaired division as our dependent variable.

Procedure

We adapt the two-page scenario to an online platform using Qualtrics. Participants must click through the study in sequential order and cannot return to change prior answers or reread prior information, nor can they skip ahead before answering each question. The general format of the study is to present the "current-year" situation leading up to the impairment decision, and then to present a second-year investment scenario to participants where we elicit the primary dependent variable. Participants read the scenario, which indicates that they are the current CEO of a consumer electronics firm with four different divisions: handheld devices, computers and laptops, headphones and speakers, and flat-panel displays. Historically, revenues and costs have been approximately equal across these four divisions. Participants are informed that competitors have recently achieved superior innovation in the flat-panel display area, such that they expect the future performance of that division to be below the current book value of assets. As a result, either the participant or the previous CEO, depending on the responsibility manipulation, must impair the assets of that division by $235 million dollars. In the not-responsible condition, participants learn that the previous CEO made an equal allocation across the three asset classes of small displays, medium displays, and large displays. In the responsible condition, participants must make the allocation themselves. Participants are presented with identical firm and division information regardless of their impairment decision responsibility. This design allows the impairment decision responsibility manipulation to affect participants' perceived responsibility for the impairment decision without changing the financial implications of subsequent decisions. Results indicate that participants with impairment decision responsibility tended to make equal allocations to each asset class (32 percent, 34 percent, and 33 percent), such that the allocation of the impairment is equivalent across impairment decision responsibility conditions.

In the second year, in their capacity as current CEO, participants must allocate $800 million in R&D funds to each of the firm's four divisions. While the information presented in the case is necessarily limited, the scenario implies that future performance could improve if the division receives additional funding that allows for R&D productivity increases, but that this is a risky proposition. We word the scenario in such a way that, ignoring our manipulations, participants, on average, would allocate approximately equal amounts across all divisions. More specifically, an argument could be made for either reducing or increasing the amount allocated to the impaired division such that, on average, allocations should even out across participants. While historically poor performance might argue for allocating less R&D money to the impaired division, the possibility that the division fell behind competitors because it was underfunded might argue for increasing R&D investment in the impaired flat-panel division.

Prior to eliciting participants' allocation decisions, we also inform them that the accounting effect of the impairment is either reversible or irreversible. In the reversible condition, we state: "This decision is reversible even though the accounting loss on these assets has already been recognized in the financial statements. If the flat-panel display division performs unexpectedly well in the future, then the value of the previously impaired assets can be increased." We further explain that any increase in value will be recorded on the balance sheet and that the gain will be reported in net income.

In the irreversible condition, we state: "This decision is irreversible and the accounting loss on these assets has already been recognized in the financial statements. Even if the flat-panel division performs unexpectedly well in the future, the value of the previously impaired assets cannot be increased." We further explain that any increase in value will *not* be recorded on the balance sheet and that the gain will *not* be reported in net income. Appendix A presents materials from the Responsible, Reversible condition with bolded text where words varied between conditions.

After allocating the R&D money, participants answer a number of personality questions, followed by manipulation check questions. Because prior work indicates that self-monitoring can affect investment decisions (Kadous and Sedor 2004; Seybert 2010), we collect participants' self-monitoring scores using Snyder and Gangestad's (1986) self-monitoring scale to potentially include as a covariate. We also collect participants' need for consistency score, based on the scale developed by Cialdini, Trost, and Newsom (1995), to potentially include as a covariate, because the relationships between individuals' past decisions and future decisions could be affected by this trait. Neither of these measures is significant as a main or interaction effect, so we do not discuss them further.

Results

H1 predicts that managers responsible for the impairment decision will invest more (less) in the impaired division when the accounting effect of the impairment is reversible (irreversible) compared to managers who are not responsible for the impairment decision. We utilize a standard ANOVA to test for our predicted interaction.[9] Table 1, Panel A presents the pattern of means across conditions, while Panel B displays the ANOVA results. Figure 3 presents our results graphically. The reversibility by responsibility interaction is significant ($F(1,62) = 7.26$, $p = 0.005$, one-tailed), suggesting that responsibility for the impairment decision significantly interacts with reversibility of the impairment in a manner consistent with psychology theory.[10,11]

With regard to simple effects, H1 predicts that when the accounting effect of the impairment is *reversible,* managers will invest *more* in the impaired asset than when the impairment is *irreversible,* provided that they are responsible for the impairment decision. Simple main effects tests confirm the shape of this interaction. As presented in Table 1, Panel C, the effect of impairment reversibility on investment is significantly positive when the manager is responsible for the impairment decision ($F(1,62) = 6.92$, $p = 0.005$, one-tailed), and not significant when the manager is not responsible for the impairment decision ($F(1,62) = 1.39$, $p = 0.243$, two-tailed). Table 1, Panel C also shows that impairment responsibility has a negative effect on investment when the impairment is irreversible ($F(1,62) = 3.01$, $p = 0.044$, one-tailed), but a positive effect on investment when the impairment is reversible ($F(1,62) = 4.31$, $p = 0.021$, one-tailed).

Taken together, the results suggest that responsibility for the impairment decision significantly interacts with reversibility of the impairment in a manner consistent with psychology theory. When the accounting effect of the impairment is reversible, participants responsible for the impairment decision allocate greater R&D funds to the impaired division, consistent with them resolving their dissonance by trying to turn around the future performance of the impaired division, following Link 3 in Figure 1. When the accounting effect of the impairment is irreversible, participants responsible for the impairment allocate fewer R&D funds to the impaired division, consistent with them

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[9] An alternative analysis would be a contrast-coded ANOVA that includes a difference in the responsible treatments that is greater than the difference in the not-responsible treatments. The coding would be reversible/responsible: 3; irreversible/responsible: −3; reversible/not responsible: 1; irreversible/not responsible: −1. This contrast-coding scheme yields a contrast effect significant at $p < 0.05$ with insignificant residual variance.

[10] Eleven participants responded incorrectly to one of two manipulation check questions involving their responsibility for the impairment decision and the reversibility of the impairment. These participants are included in all analyses, although inferences are identical and our hypothesized effects are statistically stronger if they are excluded.

[11] While H1 does not specifically predict a crossover interaction, it is one of the possible shapes that the interaction may take. Although the not-responsible cells reversed direction, the difference between them is not significant.

TABLE 1

Experiment 1
Descriptive Statistics and Analysis of Variance

Panel A: Dollar Amount of R&D Funding Allocated to Flat-Panel Division Mean (Standard Deviation) [n] by Condition

	Impairment Reversibility		
Impairment Responsibility	**Reversible**	**Irreversible**	**Overall**
Not Responsible	169.4 (51.7) [17]	200.6 (73.5) [16]	184.5 (64.2) [33]
Responsible	224.4 (99.3) [16]	154.7 (73.6) [17]	188.5 (92.6) [33]
Overall	196.1 (82.0) [33]	178.0 (76.0) [33]	

Panel B: Dollar Amount of R&D Funding Allocated to Flat-Panel Division ANOVA

Source	SS	df	MS	F-statistic	p-value
Responsibility	337.1	1	337.1	0.06	0.810
Reversibility	6094.7	1	6094.7	1.05	0.308
H1: Responsibility × Reversibility	41942.6	1	41942.6	7.26	0.005*
Error	358205.1	62	5777.5		

Panel C: Dollar Amount of R&D Funding Allocated to Flat-Panel Division Simple Main Effects

Directional Hypotheses	F-statistic	p-value
Responsibility Effect for Reversible Impairment	4.31	0.021*
Responsibility Effect for Irreversible Impairment	3.01	0.044*
Reversibility Effect for Responsible	6.92	0.005*
Reversibility Effect for Not Responsible	1.39	0.243

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* Denotes one-tailed p-values.

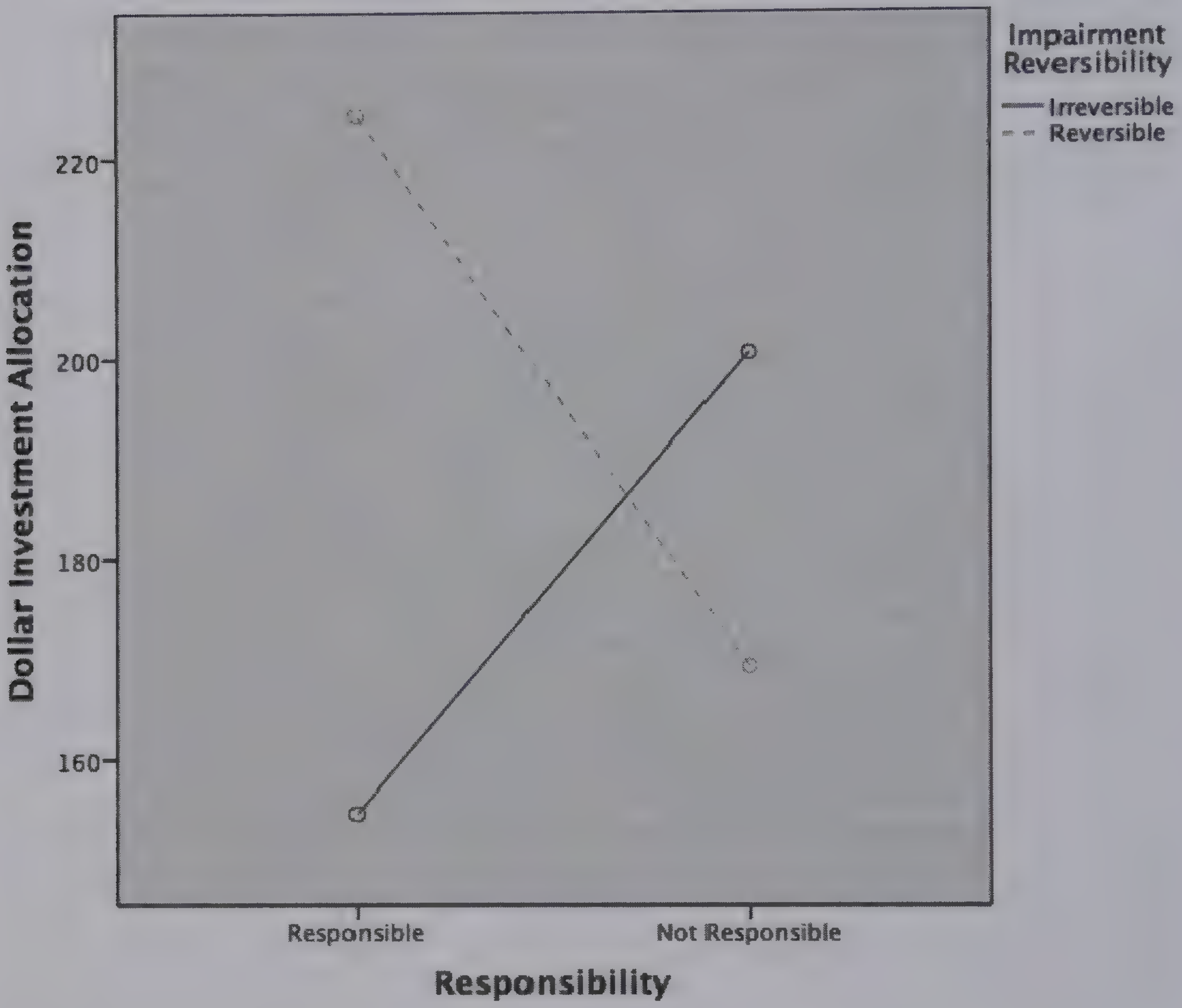
This table presents descriptive statistics and the ANOVA for the first experiment, where the dependent variable is the dollar amount of R&D funds allocated to the flat-panel division. The experiment manipulates impairment reversibility (reversible, irreversible) and impairment decision responsibility (not responsible, responsible) between participants.

resolving their cognitive dissonance by concluding or portraying that the division is a lost cause and subsequently revising their expectations for the division downward, following Link 4 in Figure 1.

Additional Analysis

Participants assumed the role of a CEO who had responsibility for both an impairment decision and an investment decision. In order to confirm that the same individual (although not necessarily the CEO) within an organization can be responsible for both impairment and future investment decisions, we separately survey 109 auditors about their clients' experiences. We survey auditors

FIGURE 3
Experiment 1
Plot of the Effects of Impairment Reversibility and Responsibility on Dollar Investment Allocation



This figure presents a plot of results for the first experiment, where the dependent variable is the dollar amount of R&D funds allocated to the flat-panel division. The experiment manipulates impairment reversibility (reversible, irreversible) and impairment decision responsibility (responsible, not responsible) between participants.

rather than CEOs and other members of management because auditors can respond based on their familiarity with multiple clients, thus giving us a richer set of information on firm behavior than if we had surveyed individual members of management.

On average, the auditors that we survey have 39.64 months of experience. When asked which members of management have had responsibility for making asset impairment decisions for accounting purposes, 28.44 percent said the CEO, 77.98 percent said the CFO, and 59.63 percent said "other," most commonly, the Controller.[12] When asked which members of management have had responsibility for making future capital investment decisions, 77.06 percent said the CEO,

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[12] Values do not add up to 100 percent because auditors were allowed to select more than one option based on dealing with multiple clients.

89.90 percent said the CFO, and 30.38 percent said "other," again, most commonly, the Controller. More importantly, when asked how common it is for the same member of management to have responsibility for both an asset impairment decision as well as future capital investment decisions related to that asset (0 = not at all common, 100 = very common), auditors' responses had a mean value of 58.43 on our scale, which is significantly above the midpoint ($p = 0.001$, one-tailed). It is important to note that this "same member of management" is more likely to be the CFO based on responses to the previous two questions. Finally, we asked auditors about the extent to which impairment decisions are subjective (0 = Not at all, 100 = Very subjective). Auditors' responses had a mean value of 67.46 on our scale, which is significantly above the midpoint ($p < 0.001$, one-tailed). This response is consistent with evidence provided by Nelson et al. (2002) that earnings management attempts involving both intangible and fixed assets are relatively frequent and often not governed by precise standards.

Combined, our results support the ideas that it is relatively common for the same member of management, usually the CFO, to have responsibility for both an asset impairment decision and a future investment decision associated with that asset, and that the initial impairment decision itself may be relatively subjective. While our results suggest that it is much more common for the CFO to be the individual who holds responsibility for both decisions, our results also support that responsibility can sometimes fall with the CEO. We asked participants in our experiment to assume the role of the CEO because the CEO is the most recognizable member of management. We believe it is unlikely that the directional effects of our manipulations would have differed had we instead asked participants to assume that they were the CFO of the firm, since the key features of our study involved informing participants that they were responsible for the impairment decision and subsequent investment decision. Having them assume the role of CFO would change some background assumptions about their position within the firm, but would not change the responsibility for decisions that they were assigned in the scenario. However, we cannot be sure that participants would have felt the same operational responsibility as a CFO that they did as a CEO because CEOs are generally more responsible for operating performance of divisions. Because feelings of responsibility are an important factor in our predictions, we note this possibility as a limitation of our study in the "Conclusion" section.

Alternative Mechanisms Affecting Investment Allocation

Our evidence is consistent with the predictions made in H1. The interaction effect predicted in H1 precludes various alternative explanations that predict only a main effect of one of our independent variables. For example, managers might believe that the reversibility of the impairment tends to decrease the amount of care taken to ensure that an asset really is impaired. If that were the case, then subsequent to recording an accounting impairment with reversible financial statement effects, managers would be less certain that the asset is performing poorly and more willing to allocate future resources to its development. This explanation predicts a main effect of the reversibility of the accounting effect of the impairment. However, it does not explain why reversibility of the impairment has a different effect depending on responsibility for the impairment decision. It also does not explain why responsibility produces a negative investment effect when the accounting effect of the impairment is irreversible.

As another example, managers might believe that the quality of operating investment decisions differs depending on whether the decision was made by the current or a previous manager. Replacing a manager may suggest that the new decision maker is more able and has access to more profitable investments than her predecessor. In this case, we would expect the current manager to believe that she is better able to turn things around in an impaired division than was the previous manager who recorded the impairment. As a result, we would expect participants to allocate more funds to the impaired division when a predecessor was responsible for the impairment decision, rather than when they were personally responsible for the impairment decision. However, this explanation predicts a main effect of managerial responsibility for the impairment decision. It does not explain why we find that personal responsibility for the impairment decision *increases* investment in the impaired asset when the accounting effect of the impairment is reversible.

Finally, examining a context where managers allocate funds to an unsuccessful venture necessitates a consideration of the theory of escalation of commitment (Staw 1976). Studies suggest that in order for escalation of commitment to occur, personal responsibility for an initial decision is necessary (see Sleesman, Conlon, McNamara, and Miles [2012] for an extensive discussion and meta-analysis of research in this area). Prior literature on escalation of commitment looks primarily at managers' initial investment decisions, and suggests that they will "throw good money after bad" and *invest more* in a failing venture to justify the initial poor investment decision. While the manager's decision in our setting is to record the impairment rather than to make the initial investment decision, our earlier discussion suggested that impairment responsibility could induce perceived responsibility for the division's poor performance. Given a reversible impairment, escalation of commitment and Link 3 in Figure 1 would work in the same direction, leading to a positive investment effect. Given an irreversible impairment, escalation of commitment would work against Link 4 in Figure 1, leading to a positive investment effect offsetting the negative investment of impairment rationalization. While it is not clear *ex ante* whether these effects are mutually exclusive, the negative investment effect of responsibility in the irreversible impairment conditions suggests that rationalization is partially at work. In summary, regardless of impairment reversibility, escalation of commitment predicts only a positive main effect of responsibility on investment. Because our hypothesis takes the form of an interaction and we find no main effect of responsibility, escalation of commitment alone cannot explain our results. In other words, while the responsibility manipulation could induce escalation of commitment in participants, the results reveal additional forces at work, and the directional nature of those forces is consistent with cognitive dissonance theory, as previously discussed.

IV. EXPERIMENT 2

Results from our first experiment indicate that managers responsible for the impairment decision increased subsequent investment when the accounting effect of the impairment was reversible, but decreased subsequent investment when the accounting effect of the impairment was irreversible. However, we did not include a cognitive measure to capture the psychological mechanism underlying these results.

In the cognitive dissonance literature, there are no standard measures of how much dissonance participants feel outside of the effects on their subsequent beliefs and behaviors. However, dissonance research has used manipulations geared toward magnifying or reducing dissonance in order to provide increased confidence that this is the psychological mechanism underlying behavior. Festinger (1957) posits that one of the main modes of dissonance reduction is the minimization of the importance of dissonant elements, which in our experimental task is represented by the conflict between perceived responsibility for the asset impairment and the need to maintain positive beliefs in managerial ability. Given individuals' strong desire to maintain positive beliefs about themselves (Gilbert et al. 1998), we expect that minimizing the responsibility for the impaired division's performance should serve as an effective mode of dissonance reduction.

Gosling et al. (2006) introduce a manipulation that allows participants to deny responsibility and, thus, reduce the effects of dissonance on subsequent beliefs. The manipulation entails asking participants how responsible they are for their prior behavior either before or after measuring the dependent variable of interest. When participants are able to deny responsibility prior to expressing subsequent beliefs or behavior, the effect of dissonance on these measures is reduced. On the other hand, when participants must express beliefs or behavior prior to denying responsibility, dissonance continues to affect these measures. In Experiment 2, we employ this manipulation using a modified version of our first experiment in order to test whether investment allocation biases are reduced when participants can deny responsibility for their role in overseeing the division's performance, as depicted via Link 5 in Figure 1. We predict the following:

> **H2:** There will be a larger difference in investment allocation between reversible and irreversible impairment conditions when managers cannot deny responsibility for the impaired division's performance than when they can.

Participants

Participants are 67 workers on Amazon's Mechanical Turk (AMT) with work experience in accounting, finance, or management. They have an average work experience of 13.4 years, with 28 percent reporting accounting as a primary area of experience, 19 percent reporting finance, and 81 percent reporting management. They have taken an average of 2.5 accounting and finance courses, and 14 percent have taken five or more accounting and finance courses. In summary, these participants have similar coursework in finance and accounting, but more work experience, than the participants in our first experiment. They are compensated \$0.50 for completing the ten-minute study, which approximates the average hourly rate of \$3.00 for experiments on AMT.

Design and Procedure

The design and task are nearly identical to our first experiment except for two important changes. First, all participants receive the version of the materials in which they are responsible for the asset impairment decision. The reversible and irreversible impairment conditions represent our first between-participants manipulation. Second, we introduce a new manipulation to give participants the opportunity to reduce cognitive dissonance by denying responsibility for the impaired division's poor performance. We ask all participants the following question: "As the CEO of the company, to what degree was it your fault that the division underperformed in recent years and thus suffered the asset impairment?" The response scale ranges from 1 = Not at all my fault, to 6 = Entirely my fault. Half of the participants answer this question immediately prior to making the investment allocation, and the other half answer it immediately after making the allocation.

When participants have a chance to answer the question *before* making the investment allocation, they can reduce their feelings of cognitive dissonance by denying responsibility for the impaired division's poor performance and providing responses on the lower end of the response scale, where 1 = Not at all my fault [for the impairment]. Since their cognitive dissonance is reduced by being given the chance to deny responsibility, they have less need to reduce cognitive dissonance via the subsequent allocation decision, and we expect to see smaller differences in allocation decisions between the reversible and irreversible asset impairment conditions. Alternatively, when participants answer the question *after* making the investment allocation, they do not have the opportunity to deny responsibility in order to reduce cognitive dissonance before the allocation decision is made. Thus participants' allocation decisions should be similar to those observed in our responsible conditions in Experiment 1. That is, participants

TABLE 2

Experiment 2
Descriptive Statistics and Analysis of Variance

Panel A: Dollar Amount of R&D Funding Allocated to Flat-Panel Division Mean (Standard Deviation) [n] by Condition

	Impairment Reversibility		
Responsibility Denial	**Reversible**	**Irreversible**	**Overall**
Denial	196.8 (97.7) [19]	201.1 (107.7) [14]	198.6 (100.4) [33]
No Denial	222.2 (113.0) [16]	156.4 (87.8) [18]	187.4 (104.4) [34]
Overall	208.4 (104.2) [35]	175.9 (98.0) [32]	

Panel B: Dollar Amount of R&D Funding Allocated to Flat-Panel Division ANOVA

Source	SS	df	MS	F-statistic	p-value
H2: Planned Contrast	31446.6	1	31446.6	3.07	0.042*
Residual	7502.4	2	3751.2	0.37	0.692
Error	645308.2	63	10243.0		

* Denotes one-tailed p-values.

This table presents descriptive statistics and the contrast-coded ANOVA for the second experiment, where the dependent variable is the dollar amount of R&D funds allocated to the flat-panel division. The experiment manipulates impairment reversibility (reversible, irreversible) and opportunity for responsibility denial (no denial, denial) between participants.

will invest more in the impaired division when the impairment is reversible than when it is irreversible.

Results

H2 predicts that allowing denial of responsibility for the impaired division's poor performance will reduce the investment biases exhibited in our first experiment. The difference in investment allocation when the impairment is reversible versus irreversible should be larger when participants are unable to deny responsibility for the impaired division's poor performance. Because our hypothesis involves a difference-in-differences with no predicted crossover, we utilize planned contrast coding with the following weights: 3 (reversible, no denial); −3 (irreversible, no denial); 1 (reversible, denial); −1 (irreversible, denial). Table 2, Panel A presents the pattern of means across conditions and Panel B presents the contrast-coded ANOVA results.[13] Figure 4 presents our results graphically. The interaction contrast is significant ($F(1,63) = 3.07$, $p = 0.042$, one-tailed) and of the

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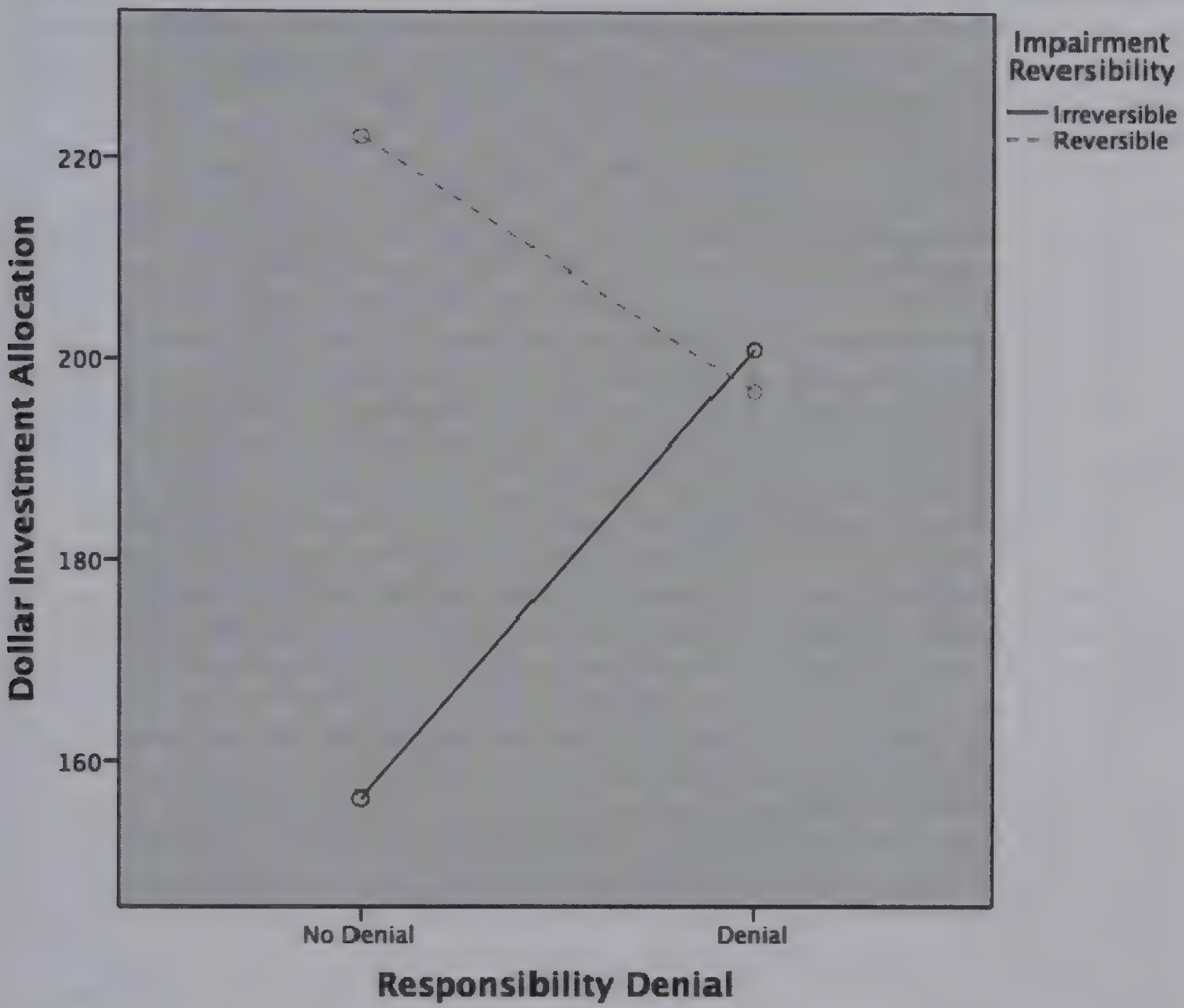
[13] Seven participants responded incorrectly to a manipulation check question involving the reversibility of the impairment. These participants are included in all analyses, although inferences are identical and our hypothesized effects are statistically stronger if they are excluded.

FIGURE 4
Experiment 2
Plot of the Effects of Impairment Reversibility and Opportunity for Responsibility Denial on Dollar Investment Allocation



This figure presents a plot of results for the second experiment, where the dependent variable is the dollar amount of R&D funds allocated to the flat-panel division. The experiment manipulates impairment reversibility (reversible, irreversible) and opportunity for responsibility denial (no denial, denial) between participants, holding constant that participants are responsible for the asset impairment decision.

generally predicted form. R&D allocation to the impaired division is greater when the impairment is reversible than when it is irreversible when participants cannot deny responsibility (μ_{rev} = \$222 versus μ_{irr} = \$156, t(33) = 1.91, p = 0.033, one-tailed). However, R&D allocation does not differ between reversible and irreversible settings when participants can deny responsibility (μ_{rev} \$197 versus μ_{irr} = \$201, t(32) = –0.12, p = 0.907).[14]

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[14] The mean responsibility rating was 4.03 when we asked the question after investment allocation ("No-Denial" condition), which corresponds to the level "moderately my fault." The mean was 3.69 when we asked the question before investment allocation ("Denial" condition), which is between "slightly my fault" and "moderately my fault," although the difference is not significant (p = 0.177).

Taken together, the results suggest that providing participants the opportunity to deny responsibility for the impaired division's poor performance significantly interacts with impairment reversibility in a manner consistent with psychology theory. In our second experiment, the investment biases documented in our first experiment are present again, but only when participants cannot deny responsibility for the division's poor performance. When participants can deny responsibility for the poor performance prior to making the investment allocation, impairment reversibility has no effect on investment, suggesting that denial of responsibility provided an alternative avenue for dissonance reduction.

V. CONCLUSION

We find that participants assuming the role of a CEO who *is* responsible for the decision to record an asset impairment subsequently invest more (less) in the impaired division when the impairment is reversible (irreversible), as compared to participants assuming the role of a CEO who *is not* responsible for the decision to record the asset impairment. We hypothesize that this effect is a result of cognitive dissonance reduction and provide evidence consistent with this process. Specifically, managers who should not experience cognitive dissonance, as well as those who are allowed to reduce it prior to making their investment, do not provide different R&D allocations across the reversible and irreversible impairment settings.

Our study contributes to the current literature by identifying how a difference between U.S. GAAP and IFRS accounting for asset impairments affects managers' operating decisions. Our study informs managers, investors, and other market participants that different levels of investment may result based solely on the reversibility of asset impairments under U.S. GAAP and IFRS regimes. Regulators may want to consider this impact when deciding whether to adopt IFRS treatment of asset impairments.

However, our results may also extend to accounting settings outside of asset impairment where a single decision maker has responsibility for an accounting choice as well as reported performance. For example, recording a contingent environmental liability is a significant event that reduces the reported performance for which managers are held responsible. The act of recording a contingent liability then creates a conflict between decreasing the company's bottom line and managers' desire to maintain a positive self-image. In such circumstances, our findings suggest that managers may alter their future behavior in order to maintain a positive self-image. For example, managers may prolong the closing of an affected job site, a decision that could prove costly to the firm if damage to the site continues to be incurred. Alternatively, positive consequences are possible, such as improving future waste disposal practices or dedicating more resources to environmental protection, both of which could limit exposure to subsequent liabilities. Future research could investigate these possibilities.

Future research could also investigate whether the pattern of investment differences we demonstrate is detectable in archival data. For example, do companies reporting under IFRS exhibit greater post-impairment investment than companies reporting under GAAP? Are returns per dollar of investment following an impairment lower under IFRS than under GAAP? Our second experiment suggests that one way of reducing cognitive dissonance prior to investment is to allow managers to deny responsibility for the initial impairment decision. Future experimental studies could investigate other moderators of the behavior exhibited in our study, such as practices, controls, or incentives that may affect managers' propensity to experience cognitive dissonance when making investment decisions for previously impaired assets.

Our study is subject to several limitations. First, due to time constraints, participants' task in our experiment is a simplification of an investment task that would be required to allocate R&D funds to a specific project. Prior to making an R&D allocation decision, we ask participants to assume that either they or a prior manager made a decision to impair an asset, and provide them with limited information about the impaired asset. Although these choices affect the mundane realism of our design, they should not impact the direction of participant responses. Second, we do not use managers as participants in our experiment. Given that we investigate a psychological phenomenon that we do not *a priori* expect to differently affect managers and non-managers, we believe that we have properly matched participants to the goals of our experiment (Libby et al. 2002). Third, we pay participants a flat fee in our study and, therefore, we cannot say whether our results would generalize to settings in which managers face performance-based incentives. Finally, we ask participants to assume the role of CEO when completing the study. It may be that CEOs are more susceptible to the effects demonstrated in our experiments than CFOs, since CEOs are more likely to feel an overall responsibility for each division's performance than CFOs, who are more likely to be concerned primarily with the appropriateness of accounting methods.

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APPENDIX A

Materials for Responsible, Reversible Accounting Effect of Impairment Condition[15]

You are the CEO at a large company that produces a variety of innovative consumer electronics devices. **Last year was** your first year in this role. The company has four divisions: handheld devices, computers and laptops, headphones and speakers, and flat-panel displays. Historically, the company's total costs and revenues have been approximately evenly divided between these four divisions.

The following events took place last year:

On December 31 of last year, **you were** forecasting future performance of the four divisions. Your competitors had recently been out-innovating you in flat-panel display technology, and market forces were pushing profit margins lower. In fact, **you** estimated that the current book value of the expensive equipment used to design, develop, and produce displays would not be fully recoverable in future years. *In other words,* ***you*** *expected future revenues would not be high enough to offset the cost of the assets in the flat-panel display division.*

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[15] Bolded words differ between conditions.

Because accounting standards require **you** to "impair" assets (reduce their value on the balance sheet) when future revenues are below the cost of those assets, **you** made the decision to impair the values by approximately 50 percent. As a result, **you** reduced the total value of assets for the digital display division on the company's balance sheet from $500 million to $265 million, and an impairment loss of $235 million was reported on the income statement.

Your decision for last year:

You had to decide how to allocate the impairment of the assets within the flat-panel display division. The division produces three categories of display panels with a roughly equal breakdown of historical costs and revenues. **Please write in the percentage of the $235 million impairment below next to each of the asset categories, such that the total percentage allocated across the three categories adds up to 100 percent:**

Equipment used to produce panels smaller than 32 inches: ____%
Equipment used to produce panels between 32 inches and 52 inches: ____%
Equipment used to produce panels larger than 52 inches: ____%

This decision is **reversible, even though** the accounting loss on these assets has already been recognized in the financial statements. **If** the flat-panel division performs unexpectedly well in the future, then the value of the previously-impaired assets **can** be increased. For example, if future performance implies that a value of $300 million is more appropriate than the $265 million currently recorded, then the $35 million dollar increase in value **can** be recorded in the balance sheet and **will** be recorded as a $35 million dollar gain in reported net income.

As with any projection, there is significant uncertainty regarding the future performance of the flat-panel division, and it could end up better than previously expected. The division was outperforming competitors several years ago. If the division can improve its R&D productivity in the future, then a significant increase in profitability is possible (but also uncertain).

Your decision for the current year:

This year, research and development funds must be allocated to the company's four divisions. The total budget is $800 million dollars. Your task as current CEO is to decide how much of the total budget to allocate to each division. Please write in the dollar amount to allocate to each division below:

R&D Budget for:
Flat-Panel Division: $____ million
Headphones and Speakers: $____ million
Computers and Laptops: $____ million
Handheld Devices: $____ million

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Communication and Information Sharing in Teams

Naomi R. Rothenberg
University of Alberta

ABSTRACT: This paper studies whether dissemination of private, pre-decision signals about productivity is valuable to the principal when agents work sequentially and observe each other's effort. The benefit of dissemination is that when productivity states are correlated, each agent's signal is useful as a performance measure for the other agent and for making efficient production choices. The more informative the signal is about agents' efforts, the greater the benefit of the additional performance measure, but there is a cost due to higher information rents. With no dissemination, the downstream agent learns about the upstream agent's productivity state by observing that agent's effort. The upstream agent's rents are lower because he has no incentive to free-ride on the downstream agent, who follows his effort, but there is less information on which to base the payments. The choice between dissemination and no dissemination depends on the informativeness of the signal about agents' efforts.

Keywords: *principal-agent theory; asymmetric information; communication; information sharing.*

JEL Classifications: *D82; D83; J41; M41.*

I. INTRODUCTION

An important consideration in the design of accounting information systems is to whom access should be given (Feltham and Hofmann 2012; Demski, Lewis, Yao, and Yildirim 1999). When information is disseminated, it is shared formally via an accounting system that involves verifiable, hard reporting (Ijiri 1975). When information is not disseminated, managers who work closely can observe each other's productive effort, thereby sharing information informally. This paper considers whether a firm owner should enable managers to share information

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formally or informally within organizations in which managers are privately informed about productivity in their own divisions and production is sequential.

Managers acquire private information that can be incorporated into the accounting system and potentially made public (Christensen 2010; Hofmann and Rothenberg 2013). For example, with participatory budgeting, managers collect and report private information to the owner (Antle and Eppen 1985). The budgets or other productivity information might or might not be disseminated within the firm. By investing in information systems, such as enterprise resource planning systems, an owner controls access to the information.

The information sharing between a firm's owner and its employees varies considerably in practice.[1] Whole Foods relies on the accounting system to formally share financial information with all of its employees, including compensation, sales, profit margins, store performance, and productivity information (Fishman 1996).[2] Google and Cisco both are well known for their open office design, which allows for informal information sharing between managers (Wakefield 2008; Lev-Ram 2006; Cisco 2007). The proponents of open office design claim that the benefits from increased communication and coordination arise because an open office design forces employees to interact spontaneously (Fayard and Weeks 2011). By restricting formal information sharing, employees in an open office design have an incentive to learn from coworkers. This paper considers the trade-offs related to managerial incentives that a firm owner faces in choosing whether to disseminate managers' private information when a manager can also learn by observing another manager's productive effort.

I use a principal multi-agent model featuring two risk-neutral agents, such as managers, with limited liability who work sequentially. The agents work closely so that each agent observes the other agent's action, but they cannot commit to side-contracting. Joint output depends on the agents' efforts and their productivity. Each agent privately observes the productivity state of his own task, which is useful for making an efficient effort choice. Each agent's state is *ex ante* more likely to be productive than nonproductive. Higher effort in a productive state means that it is more likely that agents produce good output, but in the nonproductive state, output does not depend on effort and is always bad. The agents' productivity states are positively, but imperfectly, correlated, so that good news for one agent is more likely to occur when the other agent has good news. Because of this positive relation between the productivity state and joint output, the output given the productivity state is more informative about the agents' efforts than the output alone.

With private information, agents earn costly rents that depend on the productivity state's informativeness about the agents' efforts. With private, pre-decision productivity information, the principal must provide incentives to an agent to work hard and communicate truthfully for each realization of the state. Therefore, compensation has to be high enough to motivate each agent to work in the state with lower productivity, which means each agent earns rents when his productivity state is high. Better-quality information about an agent's effort means stronger incentives, increased compensation, and higher rents (Christensen 1981; Penno 1984).

Agents communicate their private information to the principal, who can credibly disseminate the agents' reports about their productivity states or can choose no dissemination. By disseminating the agents' reports, the principal makes each agent's private information public, allowing them to make more efficient productive choices than with no productivity information. Further, due to the correlation, each report is informative about the other agent's performance. However, providing public, pre-decision information can be costly because it creates stronger incentives (Hofmann and Rothenberg 2013). If the principal does not disseminate the agents' reports, then only the downstream agent learns about the upstream agent's state; if the downstream agent observes the upstream agent not working hard, then he believes that the upstream agent's state is nonproductive. However, with no dissemination, the principal cannot use either agent's report as an additional performance measure in contracting with the other agent.

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[1] Some practitioners emphasize only the benefits of information sharing. For example, under Total Quality Management, sharing knowledge is considered part of the improvement process (e.g., ISO 9004). A strong proponent of transparency, Case (1995) emphasizes open-book management, which involves employees having open access to financial information.

[2] Whole Foods shares so much of its financial information with its employees that the Securities and Exchange Commission (SEC) considers all employees insiders in regard to trading of stock (Fishman 1996).

The principal might prefer not to disseminate the agents' reports, depending on the informativeness of the productivity signals. With no dissemination, the downstream agent's compensation is higher than with dissemination. The downstream agent's effort is efficient because he learns about the upstream agent's state by observing his effort. However, the principal cannot use the upstream agent's report as an additional performance measure for the downstream agent. With the correlation in the agents' productivity states, not being able to use the upstream agent's report is costly, and the more informative the upstream agent's report is about the downstream agent's performance, the cost of no dissemination increases. The upstream agent's compensation might be lower than with dissemination because of the effect of the downstream agent's actions, which weakens the upstream agent's incentives. The downstream agent not only observes the upstream agent's effort, but also "follows" the upstream agent's effort; if he observes the upstream agent not working hard, then he believes that the state is nonproductive and also chooses to not work hard. As a consequence, with joint output, the upstream agent will not free-ride on the downstream agent's effort, thereby reducing rents to the upstream agent. However, without dissemination, the principal cannot tailor the upstream agent's compensation to the downstream agent's report of his productivity state. As the informativeness of the productivity signal increases, it is more costly to withhold the downstream agent's report from the upstream agent. Thus, overall, if the informativeness of the productivity signal is sufficiently low, then the principal prefers no dissemination rather than dissemination of the agents' reports.[3]

Despite the potential benefits of information sharing when the agents are privately informed, the principal might prefer both agents to be uninformed. Similar to single-agent settings, the principal trades off the cost of inefficient production in nonproductive states when agents are uninformed with the cost of information rents when agents are privately informed (Christensen 1981; Penno 1984; Baiman and Sivaramakrishnan 1991). The principal prefers agents to be uninformed if the informativeness of the productivity state and the correlation in the productivity environments are both high. With highly informative productivity states, the information rents are higher than the cost of inefficient production. Further, when agents are uninformed, stronger correlation between the agents' productivity states means production is more efficient because each agent's state is *ex ante* more likely to be productive than nonproductive, implying that agents are less likely to work hard in a nonproductive state.

The results have important empirical implications regarding the design of a managerial information system that provides resources for divisional managers to produce private productivity signals that are useful for both decision-making and performance measurement. Most importantly, among firms that do install such systems and where managers can observe each other's tasks, there is a distinction between firms that allow divisions to access signals about other divisions and those that restrict access. The model suggests that for a given correlation, firms will restrict access to

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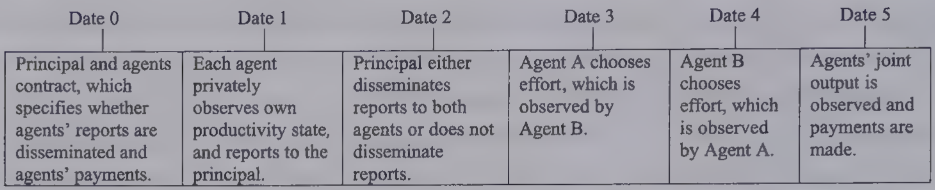
[3] The principal might prefer to disseminate information to only the upstream agent. In this case, similar trade-offs occur as between no dissemination and dissemination to both agents. A slight difference is that the upstream agent's compensation is always lower than with dissemination; the effect of the downstream agent's action and being able to base the payment on both agents' reports means a lower payment. The principal never prefers to disseminate information to only the downstream agent, because the downstream agent would then have no incentive to "follow" the upstream agent. With no benefit in a reduced payment to the upstream agent, the principal prefers dissemination to both agents rather than to only the downstream agent. Thus, the key is in restricting information to the downstream agent to provide an incentive for him to learn by observing the upstream agent's actions.

FIGURE 1
Timeline of Events



Date 0	Date 1	Date 2	Date 3	Date 4	Date 5
Principal and agents contract, which specifies whether agents' reports are disseminated and agents' payments.	Each agent privately observes own productivity state, and reports to the principal.	Principal either disseminates reports to both agents or does not disseminate reports.	Agent A chooses effort, which is observed by Agent B.	Agent B chooses effort, which is observed by Agent A.	Agents' joint output is observed and payments are made.

signals about other divisions when the signals are less informative. The model also suggests that firms will choose not to install such an information system when the signals are very informative and when the divisions' productivity environments are highly correlated.

This paper is related to several streams of research. Prior work on communication in models of moral hazard focuses on finding strict benefits of communication in single-agent settings (see Lambert [2001] and Christensen and Feltham [2005] for reviews).[4] The literature on communication of private, pre-decision information with a single, risk-averse agent started with Christensen (1981), who focuses on demonstrating that the principal is not always better off with a privately informed agent. Baiman and Evans (1983) and Penno (1984) follow this work and study the value of communication when the agent has private, pre-decision information. In this work, communication is valuable because of improved risk sharing. The present study extends this literature by considering a multi-agent setting with joint production that allows for the additional benefit of communication due to information sharing.

Laffont and Martimort (1997) focus on the effects of collusion with information sharing between agents, and Villadsen (1995) studies the benefit of delegation when the subordinates collude and share information with a supervisor. Holmström and Milgrom (1990), Itoh (1992, 1993), Ramakrishnan and Thakor (1991), and Varian (1990) focus on the benefit of the agents' ability to observe other agents' efforts, which induces more cooperation and improved risk sharing when agents side-contract. In contrast, I study the benefit of both agents' ability to observe the other agent's effort because of the information conveyed in the action, rather than due to communication or collusion between agents.

II. MODEL

The model consists of a risk-neutral principal and two risk-neutral agents with limited liability. The timeline is in Figure 1. The principal offers each agent a contract. Each agent can reject the contract, in which case the game ends, or the agent can accept the contract and join the firm. After joining the firm, each agent privately observes a perfect signal about his productivity state and communicates it to the principal. As specified in the contract, the principal either disseminates both of the agents' reports or does not disseminate the reports. Thereafter, production occurs sequentially. First, the upstream Agent A chooses his effort, which the downstream Agent B observes. Next, Agent B chooses his effort, which Agent A observes. Finally, the performance evaluation system produces a joint performance measure and wages are paid as specified in the contract.

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[4] Communication is a key element of capital budgeting models that incorporate principal-agent problems (see Antle and Fellingham [1997] for a review). In these settings, the agent has private information and an incentive to misrepresent his information. This work differs from a moral hazard setting in two ways. In the capital budgeting models, the agent takes no productive action, whereas with moral hazard, the efficient action can depend on the agent's private information. Also, performance-based pay is not a consideration in capital budgeting models, whereas with moral hazard, performance-based pay is an important element of the model.

Agents can work hard, $e^i = 1$, or not work hard, $e^i = 0$, $i \in \{A,B\}$, and each agent's effort has the same cost, ce^i, $c > 0$. The agents' joint output, x_j, can be good or bad, i.e., $j \in \{g,b\}$. Each agent's productivity state, θ^i_n, can be low, intermediate, or high, i.e., $n \in \{L,M,H\}$, and each state is equally likely.

For each agent, the joint probability of the output and the productivity state, given effort, is denoted $pr(x_j, \theta^i_n | e^i)$. The relation between the output, x, and the agent's productivity state, θ, is specified by the conditional probability of x given θ and the agent's effort, and is denoted $pr(x_j|\theta^i_n, e^i) = pr(x_j, \theta^i_n|e^i)/pr(\theta^i_n)$. For notational ease, in the high and intermediate states, let $pr(x_g|\theta^i_H, 1) = p_1, pr(x_g|\theta^i_H, 0) = p_0, pr(x_g|\theta^i_M, 1) = q_1$, and $pr(x_g|\theta^i_M, 0) = q_0$. In the low state, output is always bad regardless of effort, i.e., $pr(x_g|\theta^i_L, 1) = pr(x_g|\theta^i_L, 0) = 0$.

The high state, θ_H, is more productive than the intermediate state, θ_M, which is more productive than the low state, θ_L, which implies $p_1 \geq q_1 \geq 0$ and $p_0 \geq q_0 \geq 0$. The marginal productivity of each agent's effort is higher in the high state than in the intermediate state, or $p_1 - p_0 = \Delta p > q_1 - q_0 = \Delta q > 0$. The ratio of the marginal productivity in the high state to the marginal productivity in the intermediate state, i.e., $\Delta p/\Delta q$, is a measure of how informative output is about an agent's effort, given the signal about the productivity state. The signal is not informative about an agent's effort if $\Delta p = \Delta q$ because the agent's marginal productivity is independent of the signal. However, as Δp increases and/or as Δq decreases, the signal is more informative about an agent's effort.

The joint probability of the agents' productivity states is denoted $pr(\theta^A_n, \theta^B_m)$, $n, m \in \{L,M,H\}$, and depends on the correlation between the productivity states, σ. With three equally likely states and positive, but imperfect, correlation, $\sigma \in (1/3,1)$, where $\sigma = 1/3$ means no correlation, and $\sigma = 1$ means perfect correlation. The likelihood that agents' productivity states are the same, i.e., $n = m$, is $\sigma/3$, and the likelihood that the agents' states differ is $(1 - \sigma)/6$, $n \neq m$. Throughout the paper, I use θ_{nm} as a shortcut to denote both of the agents' states, (θ^A_n, θ^B_m).

The joint probability of the output and both of the agents' productivity states is denoted $pr(x_j, \theta_{nm}|e^A, e^B)$. The conditional probability of output, given both of the agents' productivity states and both agents' choice of effort, is $pr(x_j|\theta_{nm}, e^A, e^B) = pr(x_j, \theta_{nm}|e^A, e^B)/pr(\theta_{nm})$. Given both of the agents' states, joint output is technologically independent, which implies $pr(x_j|\theta_{nm}, e^A, e^B) = pr(x_j|\theta_n, e^A)pr(x_j|\theta_m, e^B)$. Therefore, in the state θ_{HH}, the probability of good output depending on the agents' efforts is: $pr(x_g|\theta_{HH}, 1, 1) = p_1^2$, $pr(x_g|\theta_{HH},1,0) = pr(x_g|\theta_{HH},0,1) = p_1p_0$, and $pr(x_g|\theta_{HH}, 0, 0) = p_0^2$. In the state θ_{MM}, the probability of good output depending on the agents' efforts is: $pr(x_g|\theta_{MM}, 1, 1) = q_1^2$, $pr(x_g|\theta_{MM},1,0) = pr(x_g|\theta_{MM},0,1) = q_1q_0$, and $pr(x_g|\theta_{MM}, 0, 0) = q_0^2$. In the state θ_{HM}, the probability of good output depending on the agents' efforts is: $pr(x_g|\theta_{HM},1,1) = p_1q_1$, $pr(x_g|\theta_{HM},0,1) = p_0q_1$, $pr(x_g|\theta_{HM},1,0) = p_1q_0$, and $pr(x_g|\theta_{HM},0,0) = p_0q_0$. Similarly, in the state θ_{MH}, the probability of good output depending on the agents' efforts is: $pr(x_g|\theta_{MH},1,1) = q_1p_1$, $pr(x_g|\theta_{MH},0,1) = q_0p_1$, $pr(x_g|\theta_{MH},1,0) = q_1p_0$, and $pr(x_g|\theta_{MH},0,0) = q_0p_0$. If either one or both of the agents' state is low, then the output is always bad: $pr(x_g|\theta_{nL}, e^A, e^B) = pr(x_g|\theta_{Lm}, e^A, e^B) = 0$, $n \in \{L,M,H\}$, $m \in \{L,M,H\}$, $e^A \in \{0,1\}$, and $e^B \in \{0,1\}$.

The agents' incentive problem is not separable due to the correlation between the agents' productivity states. Comparing the *ex ante* likelihood of good output with no correlation to that with correlation shows that the agents are more productive in a correlated environment than in an uncorrelated environment. The *ex ante* likelihood of good output is $pr(x_g|\theta_{nm},1,1) = (1/3)[\sigma(p_1^2 + q_1^2) + (1-\sigma)p_1q_1]$. Technically, if $\sigma = 1/3$, $pr(x_g|\theta_{nm},1,1) = (1/9)(p_1 + q_1)^2$, which is less than $(1/3)[\sigma(p_1^2 + q_1^2) + (1 - \sigma)p_1q_1]$ for $\sigma > 1/3$.

The principal can base payments on the performance measure, any communication from the agents, and the information disseminated to the agents. The agents' reports are denoted $\hat{\theta}_{nm}$. With dissemination of the agents' reports, the payment can depend on output, x_j, as well as both of the agents' reports, and is denoted $w_j(\hat{\theta}_{nm})$. Throughout the paper, I assume that the principal values both of the agents' efforts sufficiently, so that with information about θ, the principal wishes the agents to work hard only when either or both of the agents' states are θ_H and θ_M. Also note that the correlation is unaffected by whether the information is public or private, or whether the principal disseminates the agents' reports.

Generally, the principal's problem is to minimize the expected payments to the agents, subject to the individual rationality constraints that ensure each agent will join the firm at the time of contracting. Given that effort is costly to the agents, the incentive compatibility constraints ensure that the agents prefer working hard to not working hard in the productive states. With private information and without the possibility of the information being verified, the agents can misreport their information. However, the incentive constraints also ensure that the agents prefer to report their information truthfully. Use of the Revelation Principle (Myerson 1982) in this setting allows me to focus on the agents' truthful reporting without loss of generality because I assume that the principal can fully commit *ex ante* to the use of the agents' reports, including the dissemination decision and the payments to the agents.[5] The principal's problem depends on whether the principal disseminates the agents' reports. With dissemination, the principal has no incentive to misreport the agents' information, and his report is truthful because it can be verified with each agent's report.[6]

III. ANALYSIS

Benchmark—Public Signals

To provide a benchmark for inefficiencies due to either information rents with private information or misallocated effort with no information, in this section, I assume agents' signals about the productivity state are verifiable. With a public signal, the principal motivates both agents to work hard only in the productive states. The principal's problem with public signals for agents $i \in \{A,B\}$ is as follows:

$$P^{Public}\ \underset{w^i \geq 0}{Min} \sum_{n,m=M,H} \sum_{j=g,b} \Big[pr\big(x_j, \theta_{nm}|1,1\big)w_j^i(\theta_{nm}) + pr\big(x_j, \theta_{nL}|0,0\big)w_j^i(\theta_{nL}) + pr\big(x_j, \theta_{Lm}|0,0\big)w_j^i(\theta_{Lm})\Big]$$

subject to:

$$\sum_{n,m=M,H} \sum_{j=g,b} \Big\{pr(x_j, \theta_{nm}|1,1)[w_j^i(\theta_{nm}) - c] + pr(x_j, \theta_{nL}|0,0)w_j^i(\theta_{nL}) + pr(x_j, \theta_{Lm}|0,0)w_j^i(\theta_{Lm})\Big\} \geq 0 \qquad \text{(IR-Public)}$$

$$\sum_{j=g,b} pr(x_j|\theta_{nm}, 1, 1)w_j^i(\theta_{nm}) - c \geq \sum_{j=g,b} pr(x_j|\theta_{nm}, 0, 1)w_j^i(\theta_{nm}), \qquad n, m \in \{M, H\} \qquad \text{(IC-Public)}$$

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[5] Generally, the Revelation Principle (Myerson 1982) implies agents' truthful reporting is without loss of generality as long as agents' message space is not restricted, contracts are not restricted, and there is a disinterested mediator with no payoffs, or a principal who fully commits to the use of the agents' information.

[6] If the principal's report cannot be verified with the agents' reports, then the principal can misrepresent the agents' reports and prefers to always report the less costly state to the other agent; from the agents' perspectives', always reporting the same information is equivalent to no information being disseminated, implying that the only possible contract the agents are willing to accept is one with no dissemination. Therefore, without verification, the principal's commitment to truthfully disseminate the agents' reports is important.

The principal's problem is to minimize the expected payments, which can depend not only on the output, but also on the signals of the productivity states. The payments are denoted $w_j^i(\theta_{nm})$, where agent i's state is θ_n, and the other agent's state is θ_m. The individual rationality constraint (IR-Public) ensures that the expected payment less the cost of working hard is at least as large as the agent's reservation wage, which I normalize to zero without any loss of generality. The incentive constraints (IC-Public) are *ex post* (i.e., after observing the signal, but before the choice of effort) and provide incentives for the agent to work hard only in the productive states, i.e., θ_{HH}, θ_{HM}, θ_{MH}, and θ_{MM}, assuming that the other agent works hard.

The solution is for each of the four incentive constraints to be binding.[7] The optimal contract is as follows. When either one agent or both of the agents' state is low, the principal pays nothing because output is bad regardless of effort and agents do not work hard. However, when both of the agents' states are productive, output depends on agents' effort. Payments under the optimal contract are:

$$w_g^i(\theta_{HH}) = \frac{c}{p_1 \Delta p},\ w_g^i(\theta_{HM}) = \frac{c}{q_1 \Delta p},\ w_g^i(\theta_{MH}) = \frac{c}{p_1 \Delta q},\ w_g^i(\theta_{MM}) = \frac{c}{q_1 \Delta q},\ \textit{and all other } w^i(\bullet) = 0. \tag{1}$$

The total expected payment, which is the sum of the expected payments to both agents, is:

$$W^{Public} \frac{(1+\sigma)c}{3}\left[\frac{p_1}{\Delta p} + \frac{q_1}{\Delta q}\right]. \tag{2}$$

The principal only makes a payment in the productive states when output is good. Payments are tailored specifically to the productivity state in that the amount depends inversely on the marginal productivity of the state in which the payment is made. Working hard means that output in the high state is more likely to be good than in the intermediate state, which implies that the payment in the high state is lower than in the intermediate state. Further, if the signal's informativeness increases, or $\Delta p/\Delta q$ increases, then the spread in the payments increases, which means that the payments in the intermediate productive states increase and the payments in the high productive states decrease.[8]

By observation, an increase in the correlation between the productivity states increases the expected payment in Equation (2). The payments in Equation (1) are independent of the correlation because the principal makes payments state by state. However, the likelihood of payment increases with the correlation because payments are made only in the productive states. Each agent's state is *ex ante* more likely to be productive than nonproductive because two of the three equally likely states are productive. As the correlation increases, the agents' states are more likely to be the same and, thus, it is more likely that the agents' states are productive. Specifically, if there is no correlation, i.e., $\sigma = 1/3$, then there are nine states, most of which are nonproductive when no payments are made. In contrast, if the correlation is perfect, i.e., $\sigma = 1$, then there are only three possible states, most of which are productive. With either perfect or zero correlation, the states are equally likely, implying that when correlation is perfect, the states are more likely to be productive than with no correlation. Therefore, when the correlation is neither perfect nor zero, as the correlation increases, the agents' states are more likely to be the same and to be productive, increasing the likelihood of the payments.

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[7] Expanded versions of all of the programs, as well as detailed solutions, are available from the author upon request.

[8] With Agent B observing Agent A's effort before choosing his own effort, the agents' game is sequential. Here, it is important to ensure that the only sequential equilibrium is for both agents to work hard in the productive states. With public information, Agent B can discern whether Agent A is working hard in the productive or the unproductive states. If Agent A deviates and chooses low effort in the productive states and Agent B also does not work hard, then both agents will be worse off than if both agents worked hard because expected compensation is higher when working hard than when not working hard (i.e., in the state θ_{HH}, $\frac{p_1 c}{\Delta p} - c > \frac{p_0^2 c}{p_1 \Delta p}$; in the state θ_{HM}, $\frac{p_1 c}{\Delta p} - c > \frac{p_0 q_0 c}{q_1 \Delta p}$; in the state θ_{MH}, $\frac{q_1 c}{\Delta q} - c > \frac{p_0 q_0 c}{p_1 \Delta q}$; and in the state θ_{MM}, $\frac{q_1 c}{\Delta q} - c > \frac{q_0^2 c}{q_1 \Delta q}$). Therefore, as the first mover, Agent A will choose to work hard in the productive states. Likewise, Agent B as the second mover will also prefer to work hard. Further, Agent A not working hard and Agent B working hard is not a sequential equilibrium because the incentive constraints ensure that Agent A prefers to work hard given that Agent B works hard.

No Signals

In this section, I determine the optimal contract with no signal about the productivity state. With both agents uninformed, the principal motivates the agents to always work hard based on the prior beliefs about productivity. The principal's problem with uninformed agents $i \in \{A,B\}$ is as follows:

$$P^{Uninformed} \underset{w^i \geq 0}{Min} \sum_{n,m=L,M,H} \sum_{j=g,b} pr(x_j, \theta_{nm}|1,1) w_j^i$$

subject to:

$$\sum_{n,m=L,M,H} \sum_{j=g,b} pr(x_j, \theta_{nm}|1,1) w_j^i - c \geq 0 \qquad \text{(IR-Uninformed)}$$

$$\sum_{n,m=L,M,H} \sum_{j=g,b} pr(x_j, \theta_{nm}|1,1) w_j^i - c \geq \sum_{n,m=L,M,H} \sum_{j=g,b} pr(x_j, \theta_{nm}|0,1) w_j^i \qquad \text{(IC-Uninformed)}$$

The principal's problem is to minimize the expected payments, which are only based on the joint output, x_j, i.e., w_j^i. The individual rationality constraint (IR-Uninformed) ensures that the agent's expected payment less the cost of working hard is at least as large as the reservation utility, which is normalized to zero. The agent's incentive constraint (IC-Uninformed) is based on the expected productivity and provides incentives to work hard given that the other agent is also working hard.

The solution involves a binding incentive constraint. The payments are:

$$w_g^i = \frac{3c}{\sigma(p_1 \Delta p + q_1 \Delta q) + 0.5(1-\sigma)(q_1 \Delta p + p_1 \Delta q)}, \quad w_b^i = 0. \tag{3}$$

The total expected payment, which is the sum of the expected payments to both agents, is:

$$W^{Uninformed} = \frac{2c\left[\sigma(p_1^2 + q_1^2) + (1-\sigma) p_1 q_1\right]}{\sigma(p_1 \Delta p + q_1 \Delta q) + 0.5(1-\sigma)(q_1 \Delta p + p_1 \Delta q)}. \tag{4}$$

The principal makes a payment only when output is good, which can only occur in the productive states. However, with no information, both of the agents work hard in the nonproductive states and in expectation the payment must be high enough to cover the cost of working hard in all of the states. Specifically, with no signal, the payment cannot be tailored to the state, but is based inversely on the average of the marginal productivity of all of the states. Compared to the payments with the public signal, there is overpayment in the state θ_{HH}, and either overpayment or underpayment in the states θ_{HM}, θ_{MH}, and θ_{MM}, depending on the informativeness of the signal, $\Delta p / \Delta q$.[9]

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[9] With uninformed agents, Agent B is not informed about productivity at the time he observes Agent A's effort. In terms of off-equilibrium behavior, Agent B prefers to not work hard if he observes Agent A not working hard. However, agents are better off working hard than not working hard, because $\frac{[\sigma(p_1^2+q_1^2)+(1-\sigma)p_1 q_1]c}{[\sigma(p_1\Delta p+q_1\Delta q)+0.5(1-\sigma)(q_1\Delta p+p_1\Delta q)]} - c > \frac{[\sigma(p_0^2+q_0^2)+(1-\sigma)p_0 q_0]c}{[\sigma(p_1\Delta p+q_1\Delta q)+0.5(1-\sigma)(q_1\Delta p+p_1\Delta q)]}$. Therefore, as the first mover, Agent A chooses to work hard and Agent B follows, which means the only sequential equilibrium is for both agents to work hard.

The effect of correlation on the expected payment in Equation (4) depends on the signal's informativeness. If the correlation increases, then the payment in Equation (3) decreases because it depends inversely on the average of the marginal productivity of all of the states, which increases as the correlation increases. With nonzero correlation i.e., $\sigma > 1/3$, there is more weight on the marginal productivity of the high state, Δp, and less on the marginal productivity of the intermediate state, Δq. Further, as the spread between Δp and Δq increases, correlation has a greater effect on decreasing the payment. In contrast, if the correlation increases, then the likelihood of the payment increases because the payment is only made in the productive states, and an increase in the correlation increases the likelihood that both of the agents' states are productive. Combining two effects, the expected payment in Equation (4) increases when the correlation increases if $\Delta p/\Delta q < p_1/q_1$, or if $q_0/q_1 < p_0/p_1$; otherwise, the expected payment in Equation (4) decreases.[10] If $\Delta p/\Delta q$ is sufficiently small, then an increase in the correlation has a relatively small effect on decreasing the payment and a larger impact on increasing the likelihood of the payment.

Private Signals and Dissemination

In this section, each agent privately observes a productivity signal about his task and communicates his observation to the principal, who disseminates each agent's report to the other agent. With dissemination and private signals, the principal motivates the agents to report truthfully, releases a verifiable report to both of the agents, and motivates agents to only work hard in the reported productive states. The principal's problem with dissemination and agents $i \in \{\text{A,B}\}$ is as follows:

$$P^D \underset{w^i \geq 0}{Min} \sum_{n,m=M,H} \sum_{j=g,b} \left[pr(x_j, \theta_{nm}|1,1)w_j^i(\hat{\theta}_{nm}) + pr(x_b, \theta_{Lm}|0,0)w_j^i(\hat{\theta}_{Lm}) + pr(x_b, \theta_{LL}|0,0)w_j^i(\hat{\theta}_{LL})\right]$$

subject to:

$$\sum_{n,m=M,H} \sum_{j=g,b} \left\{pr(x_j, \theta_{nm}|1,1)[w_j^i(\hat{\theta}_{nm}) - c] + pr(x_b, \theta_{Ln}|0,0))w_b^i(\hat{\theta}_{Ln}) + pr(x_b, \theta_{LL}|0,0)w_b^i(\hat{\theta}_{LL})\right\} \geq 0 \qquad \text{(IR-D)}$$

$$\sum_{m=M,H} \sum_{j=g,b} \left\{pr(x_j, \theta_m|\theta_n, 1,1)[w_j^i(\hat{\theta}_{nm}) - c] + pr(x_b, \theta_L|\theta_n, 0,0)w_b^i(\hat{\theta}_{nL})\right\} \geq \sum_{m=M,H} \sum_{j=g,b} \left\{pr(x_j, \theta_m|\theta_n, 1,1)[w_j^i(\hat{\theta}_{rm}) - c] + pr(x_b, \theta_L|\theta_n, 0,0)w_b^i(\hat{\theta}_{rL})\right\}$$
$$n \in \{M,H\},\ r \in \{L,M,H\} \qquad \text{(IC}^1\text{-D)}$$

$$\sum_{m=L,M,H} pr(x_b, \theta_m|\theta_L, 0,0)w_b^i(\hat{\theta}_{Lm}) \geq \sum_{m=L,M,H} pr(x_b, \theta_m|\theta_L, 0,0)w_b^i(\hat{\theta}_{rm}), \quad r \in \{M,H\} \qquad \text{(IC}^2\text{-D)}$$

$$\sum_{j=g,b} pr(x_j|\theta_{nm}, 1,1)w_j^i(\hat{\theta}_{nm}) - c \geq \sum_{j=g,b} pr(x_j|\theta_{nm}, 0,1)w_j^i(\hat{\theta}_{nm}), \quad n,m \in \{M,H\} \qquad \text{(IC}^3\text{-D)}$$

The principal's problem is to minimize the expected payments, which can be based on the joint output and both of the agents' reports, $w_j^i(\hat{\theta}_{nm})$. The individual rationality constraints (IR-D) ensure that each agent joins the firm because the expected payment less the cost of working hard is at least as large as the reservation wage. There are three sets of incentive constraints that ensure that each agent communicates truthfully and prefers to work hard in the productive states. The first set of incentive compatibility constraints (IC^1-D) ensures that the agent prefers to report the productive states θ_H and θ_M truthfully, assuming that the agent only works hard when the other agent's reported state is productive. The second set of incentive compatibility constraints (IC^2-D) ensures that the agent reports the nonproductive state θ_L truthfully, given that neither agent works hard. The third set of incentive compatibility constraints (IC^3-D) ensures that the agent works hard in the reported productive states, given the principal's dissemination of the reports of the signal and given that the other agent works hard.

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[10] Taking the derivative of the expected payment in Equation (4) with respect to σ, $\partial W^{Uninformed}/\partial\sigma > 0$ if $2({p_1}^2 + {q_1}^2 - p_1q_1)[\sigma(p_1\Delta p + q_1\Delta q) - 0.5(1-\sigma)(q_1\Delta p + p_1\Delta q)] - 2[\sigma({p_1}^2 + {q_1}^2) + (1-\sigma)p_1q_1][(p_1\Delta p + q_1\Delta q) - 0.5(q_1\Delta p + p_1\Delta q)] > 0$. This inequality reduces to $p_1\Delta q - q_1\Delta p > 0$, or $q_0/q_1 < p_0/p_1$.

The solution involves a binding incentive constraint to work hard in the intermediate state and binding incentive constraints to communicate the high and intermediate states truthfully. Payments are:

$$w_g^i(\hat{\theta}_{HH}) = w_g^i(\hat{\theta}_{MH}) = \frac{c}{p_1 \Delta q}, \quad w_g^i(\hat{\theta}_{HM}) = w_g^i(\hat{\theta}_{MM}) = \frac{c}{q_1 \Delta q}, \quad \text{and all other } w^i(\bullet) = 0. \tag{5}$$

The total expected payment, which is the sum of the expected payments to both agents, is:

$$W^D = \frac{(1+\sigma)(p_1 + q_1)c}{3\Delta q}. \tag{6}$$

The principal makes a payment only if output is good and each agent's payment depends on the other agent's report. Compared to the benchmark of the public signal, the only inefficiencies are due to information rents in the high state. Starting with the incentive constraints for effort, (IC^3-D), the payment to motivate agents to work hard in the intermediate state with the lower marginal productivity must be more than the payment to motivate the agents to work hard in high state. From the communication constraints, (IC^1-D) and (IC^2-D), each agent must be paid the same for a report of either the high or the intermediate state, i.e., $w_g^i(\hat{\theta}_{HH}) = w_g^i(\hat{\theta}_{MH})$ and $w_g^i(\hat{\theta}_{HM}) = w_g^i(\hat{\theta}_{MM})$. Otherwise, if either agent observes the high state, then that agent has an incentive to report the intermediate state because the payment for a report of the intermediate state is higher than the payment for the report of the high state. However, agents do not have an incentive to over-report the low state because output is always bad in the low state.[11,12]

By observation, the expected payment in Equation (6) increases with the correlation, while the payments are independent of the correlation between the agents' productivity states. This is similar to the public signal where payments are tailored to the state. With privately informed agents and dissemination, the payments in Equation (5) are independent of the correlation. However, as previously discussed, the likelihood of payment increases with the correlation because with higher correlation, it is more likely that both agents' states are productive, in which case a payment is made.

With private information and dissemination, agents are productively efficient, but allowing agents to access the information is costly to the principal. Given these costly information rents, the principal might be better off with agents who have no information. The following proposition

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[11] The principal can commit to make any payment for both good output and a report of the low state with no effect on the expected payment because there is no chance of good output in the low state.

[12] In the productive states, given that Agent A does not work, Agent B may prefer to also not work hard. However, this is not a sequential equilibrium because in each of the productive states, both agents are better off working hard than not working hard (i.e., in the reported state $\hat{\theta}_{HH}$, $\frac{p_1 c}{\Delta q} - c > \frac{p_0^2 c}{p_1 \Delta q}$; in the reported state $\hat{\theta}_{HM}$, $\frac{p_1 c}{\Delta q} - c > \frac{p_0 q_0 c}{q_1 \Delta q}$; in the reported state $\hat{\theta}_{MH}$, $\frac{q_1 c}{\Delta q} - c > \frac{p_0 q_0 c}{p_1 \Delta q}$; and in the reported state $\hat{\theta}_{MM}$, $\frac{q_1 c}{\Delta q} - c > \frac{q_0^2 c}{q_1 \Delta q}$). Therefore, as the first mover, Agent A will choose to work hard and Agent B will also work hard, implying that the only sequential equilibrium is for both agents to work hard.

considers whether the principal prefers the agents to be uninformed or to be privately informed with dissemination of the agents' private information:

Proposition 1: There exists a threshold, $\beta(\sigma, p_1, q_1)$, such that if $\Delta p/\Delta q \geq \beta(\sigma, p_1, q_1)$, then the principal prefers that both agents be uninformed rather than both agents be privately informed with dissemination.

Proof: All proofs, including definitions of the thresholds, are in Appendix A.

Comparing the expected payment with private information and dissemination in Equation (6) to that with no information in Equation (4), the principal prefers that the agents are uninformed if the informativeness of the signal is sufficiently large. The greater Δp is relative to Δq, the more informative the productivity signal is about the agents' efforts, which increases information rents. Information rents arise because with private information, the payment for a report of the high state must be based on the marginal productivity of the intermediate state, Δq, and is higher than tailoring the payment to the state and basing it on the higher marginal productivity, Δp. Further, the more informative the signal, the less costly the inefficient production with uninformed agents. The more informative the signal, the lower the payment due to a higher average of the marginal productivity, with more weight on the marginal productivity of the high state, Δp, and less weight on the marginal productivity of the intermediate state, Δq.

The correlation in the agents' productivity states also affects whether the principal prefers that the agents are uninformed or privately informed with dissemination because the threshold in Proposition 1, $\beta(\sigma, p_1, q_1)$, depends on the correlation. The following corollary states the comparative static result of the effect of correlation on the principal's preference for uninformed agents versus privately informed agents with dissemination:

Corollary 1: An increase in the correlation between the agents' productivity states decreases $\beta(\sigma, p_1, q_1)$, the threshold of the signal's informativeness for which the principal prefers that the agents be uninformed rather than privately informed with dissemination.

If the correlation increases, holding everything else equal, with a lower threshold for the signal's informativeness, then the principal is more likely to prefer that the agents are uninformed rather than privately informed with dissemination. Regardless of whether agents are uninformed or privately informed, the principal makes the payment in the same states, which means that increasing the correlation has the same effect on the likelihood of payment. However, the payment to uninformed agents in Equation (3) decreases because there is more weight on the productive states, which increases the average of the marginal productivity. In contrast, with privately informed agents and dissemination, the payments are state by state and there is no effect of correlation on the payments in Equation (5). Therefore, increasing the correlation reduces the cost of inefficient production and is always strictly more beneficial with uninformed agents than with privately informed agents and dissemination. Combining the results in Proposition 1 and Corollary 1, for a sufficiently high correlation, the informativeness of the signal does not have to be that high for the principal to prefer that the agents are uninformed rather than privately informed with full dissemination.

The setting here is similar to new product design in a firm using an open office plan, in which early marketing efforts are aimed at developing new product ideas and determining feasibility based on consumer research, while engineers subsequently design and develop the product. In this environment, marketing research and product development tasks are likely to be performed by different managers due to the different skill sets involved, and with an open office design, the managers can observe each other's tasks. Additionally, a failure in either the early marketing research or the subsequent development phase means an unsuccessful product. The firm owner can provide resources to both marketing and engineering managers to produce and communicate an early estimate of the likelihood of success of their task. With information about the likelihood of success of both tasks, both managers make more efficient decisions. If these estimates cannot be verified, the managers are more informed about the likelihood of success than the firm owner and the firm owner must pay information rents. When the managers' tasks have a larger effect on the project's success when the estimate of the likelihood of success is high, i.e., the estimate is highly informative about managers' tasks, then the firm owner prefers not to pay rents, but rather to incur the cost of inefficient effort when the managers are uninformed. Further, as the correlation between the likelihood of success in the early marketing phase and the product development phase increases, or as the complementarity increases, the chance of a high likelihood of success in both phases increases. This means that the managers are more likely to earn rents, and the firm owner is more likely to prefer that the managers do not produce estimates about the likelihood of success.

Private Signals and No Dissemination

In this section, the principal does not disseminate the agents' reports about their productivity state. With no dissemination, information sharing can occur informally from Agent A to Agent B because Agent B is uninformed about Agent A's state at the time he observes Agent A's action. With only one period, Agent A's observation of Agent B's effort has no impact. I assume that the agents cannot commit to side contracts or collusive agreements, and given the contract from the principal, the agents have no incentive to communicate with each other. When the agents are indifferent, I assume that they follow the principal's preference, which is not to communicate with each other.[13]

Starting with Agent B's incentives and using backward induction, assume that the contract to Agent A provides incentives to work hard only when he observes his state is θ_H or θ_M. After observing Agent A's effort, Agent B can update his beliefs about Agent A's state or ignore any information about Agent A's state. I determine the optimal contract assuming the principal wants Agent B to update his beliefs about Agent A's state from Agent A's effort choice, and then compare that contract to one where Agent B ignores Agent A's effort. If Agent B observes Agent A not working hard, then Agent B believes that Agent A's state is θ_L. In this event, the principal prefers that Agent B also does not work hard. If Agent B observes Agent A working hard, then he believes that Agent A's state is either θ_H or θ_M, and updates his prior beliefs.

Next, considering Agent A's incentives, Agent A should prefer to work hard in the states θ_H and θ_M, but not in the state θ_L. If Agent A works hard, then Agent B works hard only if Agent B's state is θ_H or θ_M. If Agent A does not work hard, then Agent B follows suit. The principal's problem with no dissemination is as follows:

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[13] With no side contracting and with payments based on joint output, the only credible communication between agents is whether that agent's state is low. If either agent's state is low, then output is always bad regardless of effort and neither agent has an incentive to over-report the state. However, if either agent's state is high, then both agents have an incentive to report that the state is the intermediate state. Given the contract with no dissemination (which will be determined subsequently in this section), agents have no incentive to communicate this information to each other. I will also demonstrate that the principal never prefers to offer a contract that gives agents an incentive to communicate directly prior to making productive choices.

$$P^{ND}\quad \underset{w^A\geq 0, w^B\geq 0}{Min} \sum_{n,m=M,H}\sum_{j=g,b}\Big\{pr(x_j,\theta_{nm}|1,1)[w_j^A(\hat{\theta}_n)+w_j^B(\hat{\theta}_m)]+pr(x_j,\theta_{nL}|1,0)[w_j^A(\hat{\theta}_n)$$
$$+w_j^B(\hat{\theta}_L)]+\sum_{m=L,M,H}\sum_{j=g,b}pr(x_j,\theta_{Lm}|0,0)[w_j^A(\hat{\theta}_L)+w_j^B(\hat{\theta}_m)]\Big\}$$

subject to:

$$\sum_{n,m=M,H}\sum_{j=g,b}\Big\{pr(x_j,\theta_{nm}|1,1)w_j^A(\hat{\theta}_n)+pr(x_j,\theta_{nL}|1,0)w_j^A(\hat{\theta}_n)-c\Big\}$$
$$+\sum_{m=L,M,H}\sum_{j=g,b}pr(x_j,\theta_{Lm}|0,0)w_j^A(\hat{\theta}_L)\geq 0 \qquad \text{(IR-A, ND)}$$

$$\sum_{n,m=M,H}\sum_{j=g,b}\Big\{pr(x_j,\theta_{nm}|1,1)[w_j^B(\hat{\theta}_m)-c]+pr(x_j,\theta_{nL}|1,0)w_j^B(\hat{\theta}_L)\Big\}$$
$$+\sum_{m=L,M,H}\sum_{j=g,b}pr(x_j,\theta_{Lm}|0,0)w_j^B(\hat{\theta}_m)\geq 0 \qquad \text{(IR-B, ND)}$$

$$\sum_{m=M,H}\sum_{j=g,b}\Big\{pr(x_j,\theta_m|\theta_n,1,1)w_j^A(\hat{\theta}_n)+pr(x_j,\theta_L|\theta_n,1,0)w_j^A(\hat{\theta}_n)\Big\}-c$$
$$\geq\sum_{m=M,H}\sum_{j=g,b}\Big\{pr(x_j,\theta_m|\theta_n,e,e)w_j^A(\hat{\theta}_r)+pr(x_j,\theta_L|\theta_n,e,0)w_j^A(\hat{\theta}_r)\Big\}-ce$$
$$n\in\{M,H\}, r\in\{L,M,H\}, e\in\{0,1\} \qquad \text{(IC}^1\text{-A, ND)}$$

$$\sum_{m=L,M,H}\sum_{j=g,b}pr(x_j,\theta_m|\theta_L,0,0)w_j^A(\hat{\theta}_L)\geq\sum_{m=L,M,H}\sum_{j=g,b}pr(x_j,\theta_m|\theta_L,0,0)w_j^A(\hat{\theta}_r),\ r\in\{M,H\}$$
$$\text{(IC}^2\text{-A, ND)}$$

$$\sum_{n=M,H}\sum_{j=g,b}pr(x_j,\theta_n|\theta_m,1,1)w_j^B(\hat{\theta}_m)-c\geq\sum_{n=M,H}\sum_{j=g,b}pr(x_j,\theta_n|\theta_m,1,e)w_j^B(\hat{\theta}_r)-ce$$
$$m\in\{M,H\}, r\in\{L,M,H\}, e\in\{0,1\} \qquad \text{(IC}^1\text{-B, ND)}$$

$$\sum_{n=L,M,H}\sum_{j=g,b}pr(x_j,\theta_n|\theta_L,0,0)w_j^B(\hat{\theta}_L)\geq\sum_{n=L,M,H}\sum_{j=g,b}pr(x_j,\theta_n|\theta_L,0,0)w_j^B(\hat{\theta}_r),\ r\in\{M,H\}$$
$$\text{(IC}^2\text{-B, ND)}$$

The principal minimizes the expected payments, which can depend on output and the agent's report, i.e., $w_j^A(\hat{\theta}_n)$ and $w_j^B(\hat{\theta}_m)$. The individual rationality constraints (IR-A, ND) and (IR-B, ND) ensure that at the time of contracting, the expected payment less the cost of working hard is at least weakly greater than the agent's reservation wage. There are two sets of incentive constraints for each agent. For Agent A, the first set of incentive constraints (IC^1-A, ND) ensures that Agent A reports the states θ_H and θ_M truthfully and prefers to work hard. Note that the right-hand side of this set of constraints differs from the usual multi-agent incentives because if Agent A works hard, then Agent B also works hard as long as Agent B's state is not θ_L. On the other hand, if Agent A does not work hard, then Agent B also does not work hard. Agent A's second incentive constraint (IC^2-A, ND) ensures that Agent A reports the state θ_L truthfully, assuming that neither agent works hard. Agent B's first set of incentive constraints (IC^1-B, ND) reflect his updated beliefs after observing

Agent A's effort and ensure that Agent B reports the states θ_H and θ_M truthfully and prefers to work hard, given that he observes Agent A working hard. Agent B's second incentive constraint (IC^2-B, ND) ensures that he reports the state θ_L truthfully and does not work hard.

The solution involves a binding incentive constraint for each agent in the intermediate state. For Agent A, the payments are:

$$w_g^A(\hat{\theta}_H) = w_g^A(\hat{\theta}_M) = \frac{c}{\sigma(q_1+q_0)\Delta q + 0.5(1-\sigma)(p_1\Delta q + q_0\Delta p)}, \quad \text{and all other } w^A(\bullet) = 0. \tag{7}$$

For Agent B, the payments are:

$$w_g^B(\hat{\theta}_H) = w_g^B(\hat{\theta}_M) = \frac{0.5(1+\sigma)c}{[\sigma q_1 + 0.5(1-\sigma)p_1]\Delta q}, \quad \text{and all other } w^B(\bullet) = 0. \tag{8}$$

The total expected payment, which is the sum of the expected payments to both agents, is:

$$W^{ND} = \frac{[\sigma(p_1^2+q_1^2)+(1-\sigma)p_1q_1]c}{3[\sigma(q_1+q_0)\Delta q + 0.5(1-\sigma)(p_1\Delta q + q_0\Delta p)]} + \frac{0.5(1+\sigma)[\sigma(p_1^2+q_1^2)+(1-\sigma)p_1q_1]c}{3[\sigma q_1 + 0.5(1-\sigma)p_1]\Delta q}. \tag{9}$$

The principal makes payments to each agent only when output is good. Compared to the benchmark in which the signal is public, there are inefficiencies due to private information and inefficient production. To motivate agents to work hard, the payment in the intermediate state is greater than the payment in the high state, due to the difference in the marginal productivity in those states. However, due to private information, the payment must be the same for a report of the high state or the intermediate state, implying that agents earn rents. Furthermore, Agent A's effort is inefficient because Agent A is only informed about his own state and works hard when Agent B's state is low. However, Agent B's effort is efficient because Agent B knows whether Agent A's state is low.[14]

With no dissemination, the principal cannot use Agent A's report in contracting with Agent B. This is costly because with correlation, each agent's report is informative about the other agent's effort. Therefore, Agent B's payment is only based on Agent A's expected productivity, which means that Agent B earns rents in the high state and potentially in the intermediate state, depending on Agent A's state. In addition, the principal prefers Agent B to update his beliefs about Agent A's state rather than to ignore Agent A's effort. If Agent B ignores Agent A's effort choice, then the principal must compensate Agent B more to work inefficiently when Agent A's state is low.[15]

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[14] Agent A cannot fully signal his information to Agent B with two effort levels and three states. If there were only two states (one in which output is always bad and one in which output depends on effort) and two effort levels, then Agent A can signal all of his information to Agent B. Here, agents earn no information rents. In the high state, agents do not want to report that the state is low because good output is impossible in the low state, but possible in the high state. With no cost to private information, dissemination is the same as when the signal is public. No dissemination is always more costly than dissemination because the principal has less information on which to base payments, and Agent A's effort is inefficient. If the model were expanded to three states and three effort levels so that Agent A can fully signal his information, then agents earn information rents, and the trade-offs between dissemination and no dissemination described in the subsequent sections continue to hold. However, the model would be more complex with expanded sets of incentive constraints.

[15] Note that if agents communicated with each other directly prior to making productive choices, then Agent A's incentives would be the same as Agent B's incentives. Here, Agent B would have no incentive to "follow" Agent A's effort. Further, each agent would know whether the other agent's state is low. The expected payments to both agents would be equal to $\frac{(1+\sigma)[\sigma(p_1^2+q_1^2)+(1-\sigma)p_1q_1]c}{3[\sigma q_1+0.5(1-\sigma)p_1]\Delta q}$, which is greater than the expected payment with dissemination in Equation (6). Therefore, the principal never prefers agents to communicate directly prior to making productive choices.

In terms of Agent A's payment, there is also a cost in that the principal cannot use Agent B's report on which to base Agent A's payments. However, the benefit is that Agent A does not have an incentive to free-ride on Agent B's effort, because Agent B "follows" Agent A's effort choice. If Agent B observes Agent A not working hard, then Agent B also does not work hard. That is, Agent A's marginal productivity depends on Agent B being uninformed about Agent A's state and observing his effort choice. Thus, Agent A's marginal productivity is higher with Agent B "following," and higher marginal productivity means a lower payment.[16] Even with this benefit, Agent A earns rents in the high state if the signal is sufficiently informative, but Agent A's payment in the intermediate state is lower than with a public signal.

Without dissemination, the agents' communication to the principal is not strictly valuable. With no communication, the principal only provides incentives for the agents to work hard in the productive states and, optimally makes a payment only when the output is good. Given the agents' private information, with no communication, the payment for good output must be the same payment for good output as the payment to Agent A in Equation (7) and to Agent B in Equation (8), but the payment is independent of the agent's report. With no possibility of good output in the low state, expected payments with no communication are the same as in Equation (9).

The effect of correlation on the expected payments differs for Agent A versus Agent B. Agent A is uninformed about Agent B's state and an increase in the correlation can cause Agent A's payment to either increase or to decrease, depending on the informativeness of the signal, $\Delta p/\Delta q$. If the signal is sufficiently informative, then the payment increases if the correlation increases because more weight is put on the state with lowest productivity, i.e., θ_{MM}, which is more costly to the principal. However, higher correlation means that the likelihood of payment increases and, overall, Agent A's expected payment in Equation (9) is increasing in the correlation.[17] Agent B can discern whether Agent A's state is low and Agent B's payment in Equation (8) is increasing in the correlation. With the likelihood of making a payment also increasing in the correlation, the expected payment to Agent B in Equation (9) increases in the correlation.

With private information, rents are costly to the principal, and despite the benefit of information sharing between agents in reduced rents, there are circumstances under which the principal prefers both agents to be uninformed. This relation is characterized in the following proposition:

Proposition 2: There exists a threshold, $\chi(\sigma, p_1, p_0, q_1, q_0)$, such that if $\Delta p/\Delta q \geq \chi(\sigma, p_1, p_0, q_1, q_0)$, then the principal prefers that both agents be uninformed rather than both agents be privately informed with no dissemination.

Comparing the expected payment with no dissemination in Equation (9) to that with no information in Equation (4), if the signal is sufficiently informative about the agents' efforts, then the principal prefers that the agents are uninformed. As previously discussed, with no dissemination, Agent A earns rents if the signal is sufficiently informative. Further, to motivate Agent B to work hard in the productive states, Agent B earns rents in the high productivity states. Taken together, if the signal is sufficiently informative, then both of the agents earn rents, which are increasing in the signal's informativeness. Uninformed agents are productively inefficient. However, as the informativeness of the signal increases, the payment to uninformed agents decreases because the average of the marginal productivity in the productive states is higher. From

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[16] If Agent B does not follow Agent A's effort, then Agent A's payment would be $\frac{c}{\sigma q_1 \Delta q + 0.5(1-\sigma) p_1 \Delta p}$, which is clearly greater than the payment in Equation (7).

[17] Technically, the derivative of the expected payment to Agent A in Equation (9) with respect to σ is positive if $\sigma[\Delta q p_1(p_1{}^2 - q_1 q_0) + \Delta p q_0(p_1{}^2 + q_1{}^2)] + (1-\sigma)[\Delta q p_1(0.5 p_1{}^2 - 0.5 q_1{}^2 - q_1 q_0) + 0.5 \Delta p q_0(p_1{}^2 + q_1{}^2)] > 0$, which is true.

Propositions 1 and 2, regardless of whether the principal disseminates the agents' reports, the principal's preference for whether the agents should be uninformed rather than privately informed depends on the informativeness of the signal exceeding a threshold.

The correlation in the agents' productivity states affects whether the principal prefers agents to be uninformed rather than privately informed with no dissemination because the threshold, $\chi(\sigma, p_1, p_0, q_1, q_0)$, in Proposition 2 depends on the correlation. The following corollary formally states the comparative static result of the effect of correlation on the principal's preference for uninformed agents versus privately informed agents with no dissemination:

> **Corollary 2:** An increase in the correlation between the agents' productivity states decreases $\chi(\sigma, p_1, p_0, q_1, q_0)$, the threshold of the signal's informativeness for which the principal prefers that the agents are uninformed rather than privately informed with no dissemination.

If the correlation increases, holding everything else equal, the threshold for the signal's informativeness decreases and the principal is more likely to prefer that the agents are uninformed rather than privately informed with no dissemination. The likelihood of the payments is the same whether the agents are privately informed or uninformed, implying that the effect of correlation differs in the impact on the payments. With privately informed agents and no dissemination, the payment to both agents is tied to the marginal productivity of that agent's intermediate state, but also depends on the expectation of the other agent's state. An increase in the correlation puts more weight on both agents' states being the intermediate state, which decreases the marginal productivity and increases the payment. When agents are uninformed, payments are based on the average of the marginal productivity, and more correlation puts more weight on the high productivity state, which decreases the payment. From Corollary 2 and Proposition 2, with higher correlation, the informativeness of the signal does not have to be as high as with lower correlation for the principal to prefer uninformed agents.

Returning to the new product design example, if the firm owner does not disseminate the managers' reports about the likelihood of success, then the engineering manager in the downstream development phase has an incentive to learn by observing the manager upstream in the marketing phase. No dissemination can only occur when different managers are responsible for the upstream and downstream tasks, which seems likely given the difference in skills required for marketing versus engineering. The marketing manager has no incentive to free-ride on the effort of the downstream engineering manager. This is because when the marketing manager does not work hard, the engineering manager infers that there is no chance of success based on early consumer research and also does not work hard. With managers not working hard, the project is more likely to fail, and no bonuses are paid. Therefore, the firm owner can offer a lower bonus that still gives the marketing manager motivation to work hard if there is a chance the project is successful. The difference between the high and moderate likelihood of success must be large for the marketing manager to earn rents. Even so, with a sufficiently informative signal, the firm owner prefers that the managers do not produce an estimate of the likelihood of success in their division. Further, if the complementarity between the phases increases or the correlation increases, then the chance of high likelihood of success in both phases is greater, and managers earn more rents.

IV. COMPARISON OF INFORMATION SYSTEMS

Given the variation in information system design, I consider how the quality of the information and the correlation between an upstream and a downstream division's productivity determine the principal's preference for an information system that disseminates information to both managers, or an information system that only provides managers with information about their own division, or no information system.

Dissemination versus No Dissemination

In this section, I compare dissemination, where both of the agents learn about the other agent's productivity state from the disseminated report, to no dissemination, where only Agent B learns about Agent A's state by observing Agent A's effort. One difference between the settings is in terms of efficient production: with dissemination, both agents' efforts are efficient, and with no dissemination, only Agent A's effort is inefficient, as he works when Agent B's state is low. A second, and more important, difference between the two settings is that with no dissemination, Agent B is uninformed about Agent A's productivity state, which gives him an incentive to learn by observing Agent A's effort.

Depending on the informativeness of the signal, Agent A's expected payment might be less when there is no dissemination than with dissemination.[18] There are two factors that affect whether Agent A's expected payment is lower with no dissemination than with dissemination. The first is the effect of Agent B's actions on the incentives of Agent A. With dissemination, Agent B is informed and works hard in the productive states regardless of Agent A's actions. Thus, Agent A has an incentive to free-ride on Agent B's effort and the principal must provide incentives to Agent A to not shirk and let Agent B work hard. With no dissemination, because Agent B is uninformed and follows Agent A's effort, Agent A does not have an incentive to free-ride on Agent B's effort, which reduces Agent A's payment. The second factor is that with no dissemination, Agent A is uninformed about Agent B's state and works inefficiently, which is costly to the principal.[19]

Agent B's expected payment is higher with no dissemination because the principal cannot use Agent A's report in contracting with Agent B. Due to the correlation in the productivity environments, Agent A's report is informative about Agent B's performance, and more information on which to base payments generally means lower compensation. However, providing public, predecision information can be costly because it means stronger incentives, which increases compensation (Hofmann and Rothenberg 2013). Here, Agent B's incentives to work are unaffected by the information about Agent A's state because Agent B's effort is efficient even with no dissemination.

Hence, with no dissemination, there is a potential benefit in lower compensation to Agent A, but there is a cost in higher compensation to Agent B compared to when the agents' reports are disseminated. The following proposition formally states the result concerning the principal's preference for no dissemination:

Proposition 3: There exists a threshold, $\delta(\sigma, p_1, p_0, q_1, q_0)$, such that with privately informed agents, if $\Delta p/\Delta q \leq \delta(\sigma, p_1, p_0, q_1, q_0)$, then the principal prefers no dissemination rather than dissemination of the agents' private information.

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[18] Agent A's expected payment will be less with no dissemination than with dissemination if $\frac{\sigma(p_1^2+q_1^2)+(1-\sigma)p_1q_1}{3[\sigma(q_1+q_0)\Delta q+0.5(1-\sigma)(p_1\Delta q+q_0\Delta p)]} < \frac{0.5(1+\sigma)(p_1+q_1)}{3\Delta q}$ or if $\frac{p_1}{q_1} < \frac{0.5(1+\sigma)[\sigma(q_1^2-q_0^2)+0.5(1-\sigma)(p_1q_1-p_0q_0)]-\Delta q[\sigma q_1+0.5(1-\sigma)p_1]}{\Delta q[\sigma p_1+0.5(1-\sigma)q_1]-0.5(1+\sigma)q_1[\sigma(q_1^2-q_0^2)+0.5(1-\sigma)(p_1q_1-p_0q_0)]}$.

[19] While it is possible for the principal to disseminate information to either Agent A or Agent B, but not both agents, the principal never prefers to disseminate information only to Agent B, but may prefer to disseminate information only to Agent A. In the former case, the principal prefers dissemination to both agents rather than just Agent B, because if Agent A is uninformed about Agent B's state, then Agent A's inefficient production is costly and there is no benefit without sharing information from observation of effort. In the latter case, Agent A's effort is efficient, Agent A can share information with Agent B with his effort, and the main results of the paper continue to hold. Contact the author for the analysis when the principal only disseminates information to one agent, including comparisons to no information and to dissemination to both agents.

Comparing the expected payments with no dissemination in Equation (9) to the expected payments with dissemination in Equation (6), the principal prefers no dissemination depending on the informativeness of the signal. For Agent B, the only difference in compensation between no dissemination and dissemination is the principal's inability to use Agent A's report of his productivity state as an additional performance measure. Tailoring Agent B's payment to Agent A's report is beneficial with dissemination when Agent A's state is high because of higher marginal productivity, but costly when Agent A's state is the intermediate state because of lower marginal productivity. The less informative the signals are about agents' efforts, the less costly it is for the principal to not disseminate and to not use Agent A's report in Agent B's compensation.

With no dissemination, there is a benefit of reduced compensation to Agent A if the informativeness of the signal is sufficiently low. Similar to Agent B, the cost of not being able to use Agent B's report as a performance measure for Agent A is decreasing in the signal's informativeness. However, the benefit with no dissemination is that Agent B's observation of Agent A's effort reduces Agent A's incentives to shirk by increasing his marginal productivity, which increases with a less informative signal. Given that Agent A is privately informed, his compensation is based on his marginal productivity in the intermediate state and his priors about Agent B's state. Therefore, the less informative the signal, the higher the marginal productivity in the intermediate state compared to the high state, and the lower Agent A's compensation.

Continuing with the example from the previous section, the firm owner prefers to not disseminate the reports of the estimate of the likelihood of success when these estimates are not informative about the marketing and product development efforts. That is, the firm owner can find it beneficial to allow managers to specialize in different tasks so as to promote information sharing between managers. With no dissemination, the benefit is that the incentives to free-ride for the marketing manager are weaker, which reduces compensation, but the benefit of dissemination is in using each manager's report as an additional performance measure. If the difference in the marginal productivity of the different estimates of the likelihood of success is high enough, then the benefit of the additional performance measure is greater than the reduced incentives with no dissemination.

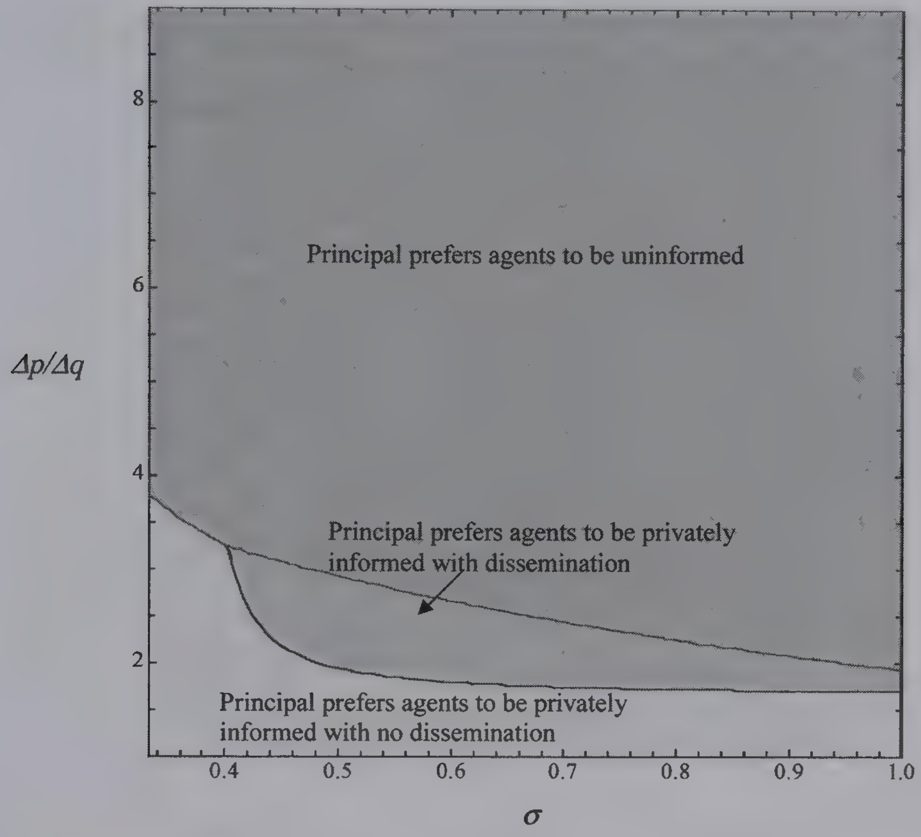
Private Signals versus No Signals

I next consider the principal's preference for the optimal information system, by combining the results from the previous sections. From Propositions 1 and 2, if the signal is highly informative, and from Corollaries 1 and 2, if the productivity states are highly correlated, then the principal prefers agents to be uninformed. As previously discussed, with either very high correlation or a highly informative signal, information rents are greater than the cost of inefficient production with no information. From Proposition 3, the principal prefers no dissemination to dissemination with a less informative signal. In this case, Agent A's inefficient production is less costly than the benefit of reduced rents, and the cost of not using Agent A's report as a performance measure for Agent B is low.

The optimal information system is best illustrated with a numerical example. Figure 2 illustrates the principal's preference for private information with dissemination, private information with no dissemination, or no information when the signal is strongly informative ($q_1 = 0.2$), depending on $\Delta p/\Delta q$ and σ. With p_0 and q_0 fixed, allowing p_1 and q_1 to vary is the same as allowing the signal's informativeness, $\Delta p/\Delta q$, to vary.

In Figure 2, the signal is more informative, and from Propositions 1 and 2, the principal more likely prefers that agents are uninformed than privately informed due to the higher rents. At lower levels of informativeness, the principal prefers no dissemination, as demonstrated in Proposition 3, which leaves the intermediate levels of informativeness, when the principal prefers dissemination.

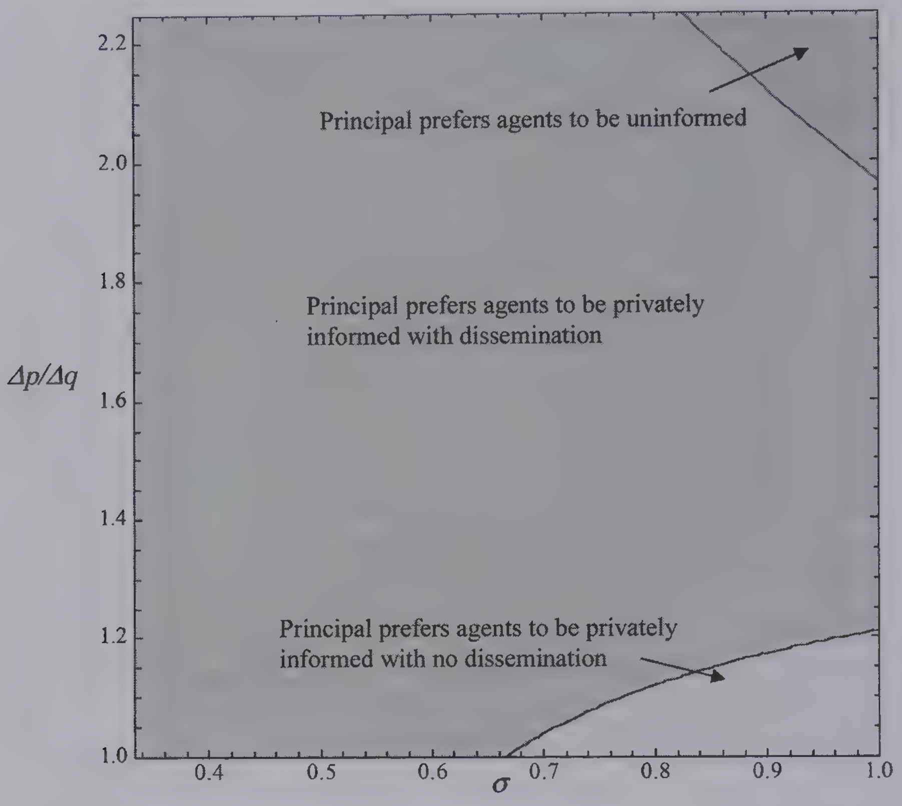
FIGURE 2
Numerical Example with High Informativeness of the Signal
$c = 1$, $q_0 = p_0 = 0.1$, $q_1 = 0.2$



The threshold for the signal's informativeness is first decreasing in the correlation and then slightly increasing in the correlation at higher levels of the correlation. At low correlation levels, the principal never prefers dissemination, but rather prefers that agents are either uninformed or privately informed with no dissemination, depending on the signal's informativeness. At such low levels of correlation, there is very little benefit in disseminating the agents' reports because they are not informative about performance. Thus, with low correlation, the principal prefers no dissemination for higher levels of informativeness. As the correlation increases, the principal's preference shifts toward dissemination as long as the informativeness of the signal is not too high or too low. If the informativeness is high, then rents are high and the principal prefers that the agents are uninformed. Conversely, if the informativeness is low, then the principal prefers no dissemination as a more efficient way to reduce rents.

Figure 3 illustrates the principal's information system choices when the signal is weakly informative ($q_1 = 0.5$). In Figure 3, the signal is less informative, which means rents are not too high and the principal is more likely to prefer that agents are privately informed than uninformed. No dissemination is beneficial with a relatively less informative signal, but the threshold for the signal's informativeness where the principal is indifferent between dissemination and no dissemination is increasing in the correlation. With an overall less informative signal and lower rents, Agent A's compensation is more likely to be higher with no dissemination than with dissemination. However, with higher correlation, it is less costly for Agent A to be uninformed about Agent B's state, similar to the way that uninformed agents are less costly when the correlation is high.

FIGURE 3
Numerical Example with Low Informativeness of the Signal
$c = 1, q_0 = p_0 = 0.1, q_1 = 0.5$



V. CONCLUSION

This paper studies information system design involving both the decision of whether to install an information system that provides private, pre-decision information to agents, and also whether the information should be disseminated to another agent. Specifically, I examine whether the principal prefers no information system, an information system that involves formal information sharing via dissemination, or a system that involves no dissemination, but promotes informal information sharing between agents. I analyze how the principal's choice depends on the quality of the information and the correlation between the divisions. For lower-quality information about the agents' performance, the principal prefers that the agents be privately informed with no dissemination, but for intermediate levels of informativeness and correlation, the principal prefers that the agents be privately informed with dissemination. For very high levels of informativeness and correlation, the principal prefers no information system.

The results of this study have implications for the design of internal information systems in multi-divisional organizations where managers work closely together, such as in firms that use open office design, or in manufacturing plants with open floor plans. While disseminating privately acquired information, such as a detailed cost analysis within the budgeting process, can improve coordination, the results also identify a potential benefit from not disseminating such information. No dissemination promotes information sharing between divisions, which can also improve coordination while reducing the rents of the manager whose information is shared. The disadvantage of no dissemination is the inability to use the information as an additional performance measure. The trade-off between dissemination and no dissemination involves the quality of the information, as well as the correlation between the divisions, both of which affect information rents.

This paper highlights the role that managers play not only in productive tasks, but also in communicating private information. An often-ignored benefit of communication is in disseminating information formally via an accounting system. However, managers who have access to information have less incentive to learn from other managers. In addition, external disclosures depend on internal communication and, thus, the results for internal information systems have implications concerning external disclosure.

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APPENDIX A

For the following proofs, let $z = \sigma p_1 + 0.5(1-\sigma)q_1$, $y = \sigma q_1 + 0.5(1-\sigma)p_1$, and $w = \sigma q_0 + 0.5(1-\sigma)p_0$.

Proof of Proposition 1

The principal prefers both agents to be uninformed rather than both agents to be privately informed with dissemination if Equation (4) $\leq$ Equation (6) or if $\Delta q[6(p_1 z + q_1 y) - (1+\sigma)(p_1 + q_1)y] \leq \Delta p(1+\sigma)(p_1+q_1)z$. This inequality can be rearranged as: $\frac{\Delta p}{\Delta q} \geq \frac{p_1[6z-(1+\sigma)y]+q_1 y(5-\sigma)}{(1+\sigma)(p_1+q_1)z}$, where $\frac{p_1[6z-(1+\sigma)y]+q_1 y(5-\sigma)}{(1+\sigma)(p_1+q_1)z} = \beta(\sigma, p_1, q_1)$. Note that the existence of a $\Delta p/\Delta q$ that satisfies this inequality is established by the numerical example in Figures 2 and 3.

Next, I show that if $\Delta p/\Delta q \geq \beta(\sigma, p_1, q_1)$, then $\Delta p/\Delta q - \beta(\sigma, p_1, q_1)$ is increasing in $\Delta p/\Delta q$ (i.e., increasing in p_1 and q_0, and decreasing in q_1 and p_0). The derivative of $\Delta p/\Delta q - \beta(\sigma, p_1, q_1)$ with respect to p_1 is non-negative if $1/\Delta q - \partial\beta/\partial p_1 \geq 0$ or if $\Delta p/\Delta q \geq \Delta p[\partial\beta/\partial p_1]$. This inequality will hold because $\Delta p[\partial\beta/\partial p_1] \leq \beta(\sigma, p_1, q_1)$, or:

$$\begin{aligned}
&\frac{p_1[6z-(1+\sigma)y]+q_1 y(5-\sigma)}{(1+\sigma)(p_1+q_1)z} \\
&\quad \geq \frac{\{6(z+\sigma p_1)-(1+\sigma)[y+0.5(1-\sigma)p_1]+0.5(1-\sigma)(5-\sigma)q_1(5-\sigma)\}\Delta p}{(1+\sigma)(p_1+q_1)z} \\
&\quad - \frac{\{p_1[6z-(1+\sigma)y]+q_1 y(5-\sigma)\}(1+\sigma)[z+\sigma(p_1+q_1)]\Delta p}{(1+\sigma)^2(p_1+q_1)^2 z^2}.
\end{aligned}$$

The latter inequality reduces to $(1+\sigma)(z-y)\{\sigma[p_1(p_1-q_1)+2p_0 q_1]+0.5(1-\sigma)p_0 q_1(p_1+q_1)\} + (5-\sigma)[(p_1{}^2+p_0 q_1)z^2+(p_1+q_1+\Delta p)q_1 yz]+0.5(3\sigma-1)(1+\sigma)q_1{}^2\Delta p[0.5(5-\sigma)(p_1+q_1)-(1+\sigma)p_1] \geq 0$, which is true. The derivative of $\Delta p/\Delta q - \beta(\sigma, p_1, q_1)$ with respect to q_1 is negative if $-\Delta p/(\Delta q)^2 - \partial\beta/\partial q_1 < 0$, or if $\Delta p/\Delta q > -\Delta q[\partial\beta/\partial q_1]$. This inequality will hold because $-\Delta q[\partial\beta/\partial q_1] < \beta(\sigma, p_1, q_1)$, or:

$$\begin{aligned}
\frac{p_1[6z-(1+\sigma)y]+q_1 y(5-\sigma)}{(1+\sigma)(p_1+q_1)z} &> - \frac{[3(1-\sigma)p_1-\sigma(1+\sigma)p_1+(5-\sigma)(y+\sigma q_1)]\Delta q}{(1+\sigma)(p_1+q_1)z} \\
&\quad + \frac{\{p_1[6z-(1+\sigma)y]+q_1 y(5-\sigma)\}(1+\sigma)[z+0.5(1-\sigma)(p_1+q_1)]\Delta q}{(1+\sigma)^2(p_1+q_1)^2 z^2},
\end{aligned}$$

which further reduces to $(1+\sigma)(z-y)p_1[\sigma p_1(p_1+q_0)+0.5(1-\sigma)q_0(p_1+q_1)]+(5-\sigma)[(p_1+q_0)p_1z^2+p_1yz\Delta q+(p_1+q_1)q_1yz]+0.5(3\sigma-1)(1+\sigma)p_1q_1\Delta q[0.5(5-\sigma)(p_1+q_1)-(1+\sigma)p_1]>0$, and which is true. By inspection, $\Delta p/\Delta q-\beta(\sigma, p_1, q_1)$ is decreasing in p_0 and increasing in q_0. ■

Proof of Corollary 1

The derivative of $\beta(\sigma, p_1, q_1)$. with respect to σ, or $\frac{\partial}{\partial\sigma}[\frac{p_1[6z-(1+\sigma)y]+q_1y(5-\sigma)}{(1+\sigma)(p_1+q_1)z}]$, will be negative if $z(1+\sigma)(p_1+q_1)\{p_1[(6+\sigma)p_1-(4+2\sigma)q_1]+q_1[(5-2\sigma)q_1-(3-\sigma)p_1]\}-(p_1+q_1)[(1+2\sigma)p_1-\sigma q_1]\{p_1[6z-(1+\sigma)y]+q_1y(5-\sigma)\}<0$. This inequality can be reduced to $z/y>\{(1+\sigma)[(1+2\sigma)p_1-\sigma q_1]\}/[2q_1(2+\sigma^2)+(5-\sigma)p_1]$ which is true because $z>y$ and the right-hand side is less than one. ■

Proof of Proposition 2

The principal prefers both agents to be uninformed rather than both agents to be privately informed with no dissemination if Equation (4) $\leq$ Equation (9) or if $\Delta qy[6(q_1y-q_0w)-\Delta qy-0.5(1+\sigma)(q_1y+q_0w)]\leq\Delta pz[\Delta qy+0.5(1+\sigma)(q_1y-q_0w)]$. This inequality will hold if $\frac{\Delta p}{\Delta q}\geq\frac{[0.5(11-\sigma)(q_1y-q_0w)-\Delta qy]y}{[0.5(1+\sigma)(q_1y-q_0w)+\Delta qy]z}$, where $\frac{[0.5(11-\sigma)(q_1y-q_0w)-\Delta qy]y}{[0.5(1+\sigma)(q_1y-q_0w)+\Delta qy]z}=\chi(\sigma,p_1,p_0,q_1,q_0)$. Note that the existence of a particular $\Delta p/\Delta q$ that satisfies this inequality is established by the numerical example in Figures 2 and 3.

Next, I show that if $\Delta p/\Delta q\geq\chi(\sigma, p_1, p_0, q_1, q_0)$, then $\Delta p/\Delta q-\chi(\sigma, p_1, p_0, q_1, q_0)$ is increasing in $\Delta p/\Delta q$ (i.e., increasing in p_1 and q_0, and decreasing in q_1 and p_0). The derivative of $\Delta p/\Delta q-\chi(\sigma, p_1, p_0, q_1, q_0)$, with respect to p_1 is non-negative if $1/\Delta q-\partial\chi/\partial p_1\geq 0$ or if $\Delta p/\Delta q\geq\Delta p[\partial\chi/\partial p_1]$. This inequality will hold because $\Delta p[\partial\beta/\partial p_1]\leq\chi(\sigma, p_1, p_0, q_1, q_0)$, or $[0.5(11-\sigma)(q_1y-q_0w)-\Delta qy][0.5(1+\sigma)(q_1y-q_0w)+\Delta qy][yz+0.25(1+\sigma)(3\sigma-1)q_1\Delta p]\geq 0.5(1-\sigma)\{[0.5(11-\sigma)q_1-\Delta q][0.5(1+\sigma)(q_1y-q_0w)+\Delta qy]-[0.5(1+\sigma)q_1-\Delta q][0.5(11-\sigma)(q_1y-q_0w)-\Delta qy]\}yz\Delta p$. This inequality holds because the left-hand side is positive and the right-hand side is negative. The derivative of $\Delta p/\Delta q-\chi(\sigma, p_1, p_0, q_1, q_0)$ with respect to q_1 is negative if $-\Delta p/(\Delta q)^2-\partial\chi/\partial q_1<0$. This inequality will hold if $\partial\chi/\partial q_1>0$, or if $[0.5(11-\sigma)(q_1y-q_0w)-\Delta qy][0.5(1+\sigma)(q_1y-q_0w)+\Delta qy]0.25(3\sigma-1)(1+\sigma)p_1+yz\{[0.5(11-\sigma)(y+\sigma q_1)-(y+\sigma\Delta q)][0.5(1+\sigma)(q_1y-q_0w)+\Delta qy]-[0.5(11-\sigma)(q_1y-q_0w)-\Delta qy][0.5(1+\sigma)(y+\sigma q_1)+(y+\sigma\Delta q)]\}>0$. The first term in this expression is clearly positive, and the second term is positive if $6(y+\sigma q_1)\Delta qy-2(y+\sigma\Delta q)\Delta qy>0$, which is true. The derivative of $\Delta p/\Delta q-\chi(\sigma, p_1, p_0, q_1, q_0)$ with respect to p_0 is negative if $-1/\Delta q-\partial\chi/\partial p_0<0$, or if $\Delta p/\Delta q>-\Delta p[\partial\chi/\partial p_0]$. This inequality will hold if $-\Delta p[\partial\chi/\partial p_0]<\frac{[0.5(11-\sigma)(q_1y-q_0w)-\Delta qy]y}{[0.5(1+\sigma)(q_1y-q_0w)+\Delta qy]z}$, or if $0.25(11-\sigma)(1-\sigma)q_0\Delta p[0.5(1+\sigma)(q_1y+q_0w)+\Delta qy]<[0.5(11-\sigma)(q_1y+q_0w)-\Delta qy][0.5(1+\sigma)(q_1y+q_0w)+\Delta qy+0.25(1+\sigma)q_0\Delta p]$, which can be rearranged as $0.5(11-\sigma)\Delta q[\sigma(q_1+q_0)+0.5(1-\sigma)p_1]>0$, and which is true. The derivative of $\Delta p/\Delta q-\chi(\sigma, p_1, p_0, q_1, q_0)$ with respect to q_0 is non-negative if $\Delta p/(\Delta q)^2-\partial\chi/\partial q_0\geq 0$, or if $\Delta p/\Delta q\geq\Delta q[\partial\chi/\partial q_0]$. This inequality will hold if $\Delta q[\partial\chi/\partial q_0]<\frac{[0.5(11-\sigma)(q_1y-q_0w)-\Delta qy]y}{[0.5(1+\sigma)(q_1y-q_0w)+\Delta qy]z}$, or if $[-0.5(11-\sigma)(w+\sigma q_0)+y]\Delta q[0.5(1+\sigma)(q_1y+q_0w)+\Delta qy]<[0.5(11-\sigma)(q_1y-q_0w)-\Delta qy][0.5(1+\sigma)(q_1y-q_0w)+\Delta qy+0.5(1+\sigma)(w+\sigma q_0)\Delta q+\Delta qy]$. This is true because $0.5(1+\sigma)(q_1y+q_0w)+\Delta qy<0.5(11-\sigma)(q_1y-q_0w)-\Delta qy$, and $[-0.5(11-\sigma)(w+\sigma q_0)+y]\Delta q<[0.5(1+\sigma)(q_1y-q_0w)+\Delta qy+0.5(1+\sigma)(w+\sigma q_0)\Delta q+\Delta qy]$. ■

Proof of Corollary 2

Let $z' = \partial z/\partial\sigma = p_1 - 0.5q_1$, $y' = \partial y/\partial\sigma = q_1 - 0.5p_1$, and $w' = \partial w/\partial\sigma = q_0 - 0.5p_0$. Also, let $a = 0.5(11 - \sigma)(q_1y - q_0w) - \Delta qy$ and $b = 0.5(1 + \sigma)(q_1y - q_0w) + \Delta qy$. Then, $a' = \partial a/\partial\sigma = 0.5(11 - \sigma)(q_1y' - q_0w') - 0.5(q_1y - q_0w) - \Delta qy'$, and $b' = \partial b/\partial\sigma = 0.5(1 + \sigma)(q_1y' - q_0w') + 0.5(q_1y - q_0w) + \Delta qy'$. Then, $\chi(\sigma, p_1, p_0, q_1, q_0)$ can be rewritten as $(y/z)(a/b)$, and the derivative of $\chi(\sigma, p_1, p_0, q_1, q_0)$ with respect to σ will be negative if $[\frac{\partial(y/z)}{\partial\sigma}]\frac{a}{b} + [\frac{\partial(a/b)}{\partial\sigma}]\frac{y}{z} < 0$. Because a/b and y/z are both positive, it is sufficient to show that $\frac{\partial(y/z)}{\partial\sigma} < 0$, and $\frac{\partial(a/b)}{\partial\sigma} < 0$. First, $\frac{\partial(y/z)}{\partial\sigma} < 0$ if $y'z - yz' < 0$. This is true because $y'z - yz' = -0.5(p_1{}^2 - q_1{}^2)$, which is clearly negative. Next, $\frac{\partial(a/b)}{\partial\sigma} < 0$ if $a'b - ab' < 0$, or if $-0.5(q_1y - q_0w)^2 - (q_1y - q_0w)\Delta qy' + (q_1y' - q_0w')\Delta q < 0$, which is true. ■

Proof of Proposition 3

The principal prefers no dissemination to dissemination if Equation (9) $\leq$ Equation (6) or if $0.5(1 + \sigma)p_1(z - y)/y \leq [0.5(1 + \sigma)(p_1 + q_1)(q_1y - q_0w) - (p_1z + q_1y)\Delta q]/(q_1y - q_0w)$. Rearranging, Equation (9) $\leq$ Equation (6) if $p_1 \leq y[(1 + \sigma)(q_1y - q_0w)(p_1 + 0.5q_1) - q_1\Delta qy]/\{z[0.5(1 + \sigma)(q_1y - q_0w) + \Delta qy]\}$. Note that the existence of a particular p_1 that satisfies this inequality is established by the numerical example in Figures 2 and 3.

Next, I show that if $p_1 \leq y[(1 + \sigma)(q_1y - q_0w)(p_1 + 0.5q_1) - q_1\Delta qy]/\{z[0.5(1 + \sigma)(q_1y - q_0w) + \Delta qy]\}$ 0, then $p_1 - y[(1 + \sigma)(q_1y - q_0w)(p_1 + 0.5q_1) - q_1\Delta qy]/\{z[0.5(1 + \sigma)(q_1y - q_0w) + \Delta qy]\}$ is increasing in p_1. The inequality can be rearranged as $p_1 \leq \frac{[0.5(1+\sigma)(q_1y-q_0w)-\Delta qy]q_1y}{(1+\sigma)(q_1y-q_0w)(0.5z-y)+\Delta qyz}$, and is increasing in p_1 if $1 - \frac{\partial}{\partial p_1}\left\{\frac{[0.5(1+\sigma)(q_1y-q_0w)-\Delta qy]q_1y}{(1+\sigma)(q_1y-q_0w)(0.5z-y)+\Delta qyz}\right\} \geq 0$. It is sufficient to show that $\frac{\partial}{\partial p_1}\left\{\frac{[0.5(1+\sigma)(q_1y-q_0w)-\Delta qy]q_1y}{(1+\sigma)(q_1y-q_0w)(0.5z-y)+\Delta qyz}\right\} \leq 0$, or $0.25(3\sigma - 1)(1 + \sigma)[0.5(1 + \sigma)(q_1y - q_0w) + \Delta qy][0.5(1 + \sigma)(q_1y - q_0w) - \Delta qy] - 0.25(3\sigma - 1)(1 + \sigma)(1 - \sigma)\,(p_1 - q_1)\Delta qq_0yw \leq 0$. This inequality holds if $p_1 \leq \frac{[0.5(1+\sigma)(q_1y-q_0w)-\Delta qy]q_1y}{(1+\sigma)(q_1y-q_0w)(0.5z-y)+\Delta qyz}$, because the numerator, $0.5(1 + \sigma)(q_1y - q_0w) - \Delta qy$, must be positive. However, if $p_1 > \frac{[0.5(1+\sigma)(q_1y-q_0w)-\Delta qy]q_1y}{(1+\sigma)(q_1y-q_0w)(0.5z-y)+\Delta qyz}$, then the numerator, $0.5(1 + \sigma)(q_1y - q_0w) - \Delta qy$, can be negative. If $0.5(1 + \sigma)(q_1y - q_0w) - \Delta qy < 0$, then $0.25(3\sigma - 1)(1 + \sigma)[\Delta qy - 0.5(1 + \sigma)(q_1y - q_0w)][0.5(1 + \sigma)(q_1y - q_0w) - \Delta qy] - 0.25(3\sigma - 1)(1 + \sigma)(1 - \sigma)\,(p_1 - q_1)\Delta qq_0yw \leq 0$. This inequality can be rearranged as $-0.5(1 - \sigma)q_1\Delta qy - q_0[0.5(1 + \sigma)q_1w + q_1y + (1 - \sigma)\,(p_1 - q_1) - 0.5(1 + \sigma)(q_1y - q_0w)q_1[0.5(1 + \sigma)(q_1y - q_0w) + \Delta qy] \leq 0$, which is clearly true. Finally, making a positive affine transformation does not affect the condition, which means that the inequality can be restated as $\frac{\Delta p}{\Delta q} \leq \frac{[0.5(1+\sigma)(q_1y-q_0w)-\Delta qy]q_1y}{[(1+\sigma)(q_1y-q_0w)(0.5z-y)+\Delta qyz]\Delta q} - \frac{p_0}{\Delta q}$, where $\frac{[0.5(1+\sigma)(q_1y-q_0w)-\Delta qy]q_1y}{[(1+\sigma)(q_1y-q_0w)(0.5z-y)+\Delta qyz]\Delta q} - \frac{p_0}{\Delta q} = \delta(\sigma, p_1, p_0, q_1, q_0)$. Regardless of the other variables, i.e., p_0, q_1, and q_0, the proof demonstrates there exists a p_1 and, therefore, a $\Delta p/\Delta q$, that satisfies the above condition. ■

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Asymmetric Responses to Earnings News: A Case for Ambiguity

Christopher D. Williams
University of Michigan

ABSTRACT: This study empirically examines the role of shocks to macro-uncertainty in shaping the responses of stock market participants to firm-specific earnings news. Specifically, I find that investors place greater weight on bad news following an increase in macro-uncertainty. By contrast, I find that investors place equal weight on both good and bad news following a decrease in macro-uncertainty. Furthermore, my findings show that these effects are more pronounced (1) for firms whose prior returns are more correlated with macro-uncertainty, (2) for firms that experience abnormally low trading volume during the earnings announcement, (3) for firms with relatively lower levels of institutional ownership, and (4) for firms with relatively higher information uncertainty. In sum, these findings provide novel empirical evidence that investors behave in a manner consistent with ambiguity aversion, with the effects strongest among unsophisticated investors.

Keywords: *ambiguity; risk; ERC.*

> What is at work is . . . "Knightian Uncertainty." When, as today, the unknown unknowns dominate, and the economic environment is so complex as to appear nearly incomprehensible, the result is extreme prudence . . . on the part of investors . . . "Better safe than sorry" is the motto."
> —Olivier Blanchard

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I. INTRODUCTION

Uncertainty impacts a myriad of individual decisions in numerous ways. In this paper, I investigate one type of uncertainty and its impact on investment decisions. Specifically, I examine how shocks to the macro-economic environment impact investor processing of firm-specific information. On a daily basis, investors process large amounts of firm-specific information that is ultimately impounded into stock prices. However, such processing does not take place in a vacuum. Instead, it occurs against the backdrop of the macro-economic environment. Consequently, shocks to the macro environment require investors to process firm-specific information *conditional* on a given shock.

The latter part of the 2000s represented a period with especially strong market turmoil, in which the effects of macro-uncertainty shocks on investor decisions became a concern for both policymakers and regulators (Bloom 2009). My study contributes to a broader understanding of how macro-level uncertainty shocks impact investor decisions. Specifically, I find that following shocks that increase macro-uncertainty, investors respond asymmetrically to subsequent earnings news, placing more weight on bad news than on good news. By contrast, I find that investors who do not experience a shock respond to good and bad news symmetrically.

As a framework for understanding how macro-uncertainty shocks may impact investor responses to firm-specific information, I draw on prior research that examines the distinction between *risk* and *ambiguity,* which is sometimes referred to as "Knightian uncertainty" (Knight 1921). Knightian uncertainty, or ambiguity, is defined as uncertainty about the *probabilities* over payoffs. Ambiguity is distinguished from risk, which is uncertainty over payoffs (Savage 1954). Another way to think about the difference between risk and ambiguity is that risk is when one does not know the outcome, but understands the odds of each outcome. Ambiguity, on the other hand, is a situation where one does not have enough information to understand the odds of each outcome. Former United States Secretary of Defense Donald Rumsfeld, during a U.S. Department of Defense (2002) news briefing, described risk as the "known unknowns" or things we know we do not know, and ambiguity as the "unknown-unknowns" or things we do not know we do not know.

Ellsberg (1961) provided the first experimental evidence that people behave as though they make a distinction between risk and ambiguity and people are adverse to ambiguity. Specifically, in the presence of ambiguity, Ellsberg (1961) finds that ambiguity-averse individuals take a conservative approach when making decisions by adopting a "worst-case" scenario by focusing on the worst-case probability distribution. This behavior is consistent with recent research in neuroeconomics that finds that when subjects are faced with decisions under ambiguity, the areas of the brain associated with fear and survival instincts are activated (Hsu, Bhatt, Adolphs, Tranel, and Camerer 2005; Smith, Dickhaut, McCabe, and Pardo 2002).

Extant literature suggests that macro-uncertainty shocks can create significant ambiguity concerns as investors try to understand the new and unfamiliar environment in which they now must operate (Epstein and Schneider 2007; Liu, Pan, and Wang 2005). This environment can directly impact investors' responses to corporate earnings news (Drechsler 2013; Epstein and Schneider 2008; Hansen and Sargent 2010; Illeditsch 2011). Specifically, in an ambiguous market environment, investors receiving earnings news behave as if they are facing a set of probability distributions over payoffs. In this situation, they are either unable or unwilling to collapse the set of distributions to a unique distribution. Given such ambiguity, investors act cautiously and take a conservative approach by choosing the worst-case distribution in the set given the news. This conservative or pessimistic approach to decision-making under ambiguity leads to asymmetric behavior in which investors place more weight on subsequent bad versus good news. In contrast, in the absence of ambiguity, investors respond symmetrically to good and bad news, consistent with decision-making under conditions of risk, where any uncertainty is over payoffs rather than probabilities.

To study the effect of macro-uncertainty shocks on investor processing of corporate earnings news, I begin with a consideration of how to measure macro-uncertainty shocks or environments where the unknown-unknowns or ambiguity are likely to be a significant force facing investors. Ideally, I would like a measure that captures large, sudden, and unexpected changes in the macro environment that can be measured just prior to the earnings announcement. For my primary proxy for macro-uncertainty shocks, I follow prior research (Bloom 2009) and use increases in the volatility index calculated on the Standard & Poor's (S&P) 100 options—which I refer to as the VIX.[1] The VIX has been calculated by the Chicago Board Options Exchange (CBOE) since 1986. Prior research has shown that large macro shocks where unknown-unknowns are likely present, including the Cuban Missile Crisis, Black Monday, the Asian Crises, 9/11, and the most recent Financial Crisis, result in large and sudden jumps in the VIX. To capture these types of changes in the VIX prior to the arrival of information, I measure the change in the VIX over the two-day period immediately preceding a three-day earnings announcement window. In doing so, I find that investors place more weight on bad news relative to good news when the VIX increases prior to the earnings announcement. Otherwise, investors respond similarly to good and bad news. In addition to changes in the VIX, I use additional proxies for macro-uncertainty shocks and ambiguous environments to check for robustness. Overall, the results are consistent with behavior predicted by theories of investor ambiguity aversion, in which investors facing ambiguity about the general macro environment take a conservative approach when responding to earnings news.

While my findings are consistent with theories of ambiguity aversion, it is possible that the change in the VIX is capturing changes in risk, changes in sentiment, or psychological biases, and the documented asymmetric response is because of these forces and not ambiguity. In response to these concerns, I consider four alternative explanations for the observed asymmetric responses to earnings news. First, my observed results may reflect the presence of volatility feedback/leverage effects, or asymmetric volatility. Indeed, Bekaert and Wu (2000) provide evidence that market-level news can create volatility feedback loops, where bad news exacerbates price responses through leverage and volatility effects, while good news dampens price responses. A key component in the Bekaert and Wu (2000) model is the requirement that the macro shock must change the firm's return covariance with the market return. In another study, Ball and Kothari (1991) document that firms' market beta changes prior to and during earnings announcement periods. To consider this explanation, I control for three potential measures of volatility responses and rerun my analysis: market beta, leverage, and market return. I find that my results are robust to controlling for these measures, suggesting the observed asymmetry is not due to asymmetric volatility responses.

A second alternative explanation is that the market may weight bad news more heavily in "good" macro-economic states and the increase in the VIX is capturing "good" macro-economic states. Conrad, Cornell, and Landsman (2002) find that investors weight bad news more heavily than good news when the macro-economy is in a relatively good state. They attribute this phenomenon to market uncertainty concerning whether bad news indicates a change in the state of the economy (Veronesi 1999). When I control for the Conrad et al. (2002) effect by including their proxy for the level of the market as a control variable, all of my results are robust to including this control.

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[1] While I refer to the volatility index calculated on the S&P 100 options as the VIX, since 2003, the CBOE has referred to this index as the VXO. In response to market demands, starting in 2003, the CBOE began calculating a volatility index on the S&P 500 options and called the new measure the VIX. I use the VXO because of its longer time-series, beginning in 1986. The index calculated on the S&P 500 options starts in 1993. My results are also robust to use of the actual VIX over the time period beginning in 1993.

A third possible explanation is that the overall level of market sentiment may influence investors to respond differentially to bad and good news and the change in VIX is capturing investor sentiment. Mian and Sankaraguruswamy (2012) show that during periods of high market sentiment, investors respond more strongly to bad versus good news. Conversely, when market sentiment is low, investors respond more strongly to good versus bad news. After including the Baker and Wurgler (2006) measure of market sentiment, I find my results are robust to this control.

Fourth, my results may reflect the fact that firms with high market-to-book ratios experience more extreme responses to bad earnings news than firms with low market-to-book ratios (Skinner and Sloan 2002). This phenomenon, coined the "torpedo effect," has been attributed to over-optimistic expectational errors (Lakonishok, Shleifer, and Vishny 1994) that are subsequently corrected at earnings announcements. While the market-to-book results are not conditional on the macro-economy, but are instead idiosyncratic, to the extent that market-to-book ratios may be correlated with macro factors, I include firm market-to-book ratios in my estimation model and the results are robust.

Finally, a large literature in psychology examines the effects of negativity bias on subsequent behavior (Baumeister, Bratslavsky, Finkenauer, and Vohs 2001; Rozin and Royzman 2001). Negativity bias occurs in a situation in which negative stimuli have a greater impact on individual responses than do positive stimuli (Akhtar, Faff, Oliver, and Subrahmanyam 2011). Note that this asymmetric treatment of good and bad news is a common link between the ambiguity aversion and negativity bias theories. This common link can also be seen on a neurological level; both ambiguity aversion and negativity bias are associated with activity in the amygdala, an area of the brain associated with fear and survival instincts (Hsu et al. 2005; Williams et al. 2009). Empirically separating ambiguity aversion and negativity bias as potential explanations for my findings is difficult given the coarseness of my data and similarities in predictions across the two theories. However, while there are many similarities between the two hypotheses, there are also differences. One important difference is that negativity bias is an *unconditional* behavior, meaning just the presence of negative news is sufficient for inducing the bias. Ambiguity aversion, on the other hand, produces asymmetrical responses to good and bad news *conditional* on ambiguity being present. Consequently, the observed symmetrical reaction to good and bad news in the absence of a shock is more supportive of ambiguity aversion than of negativity bias as an explanation for my results.

In addition to ruling out potential alternative explanations, I perform a number of cross-sectional tests motivated by ambiguity theory. First, I examine a firm's susceptibility to uncertainty shocks. The intuition behind this analysis is that firms whose returns have a greater correlation with past uncertainty shocks are more likely to be affected by current shocks. Indeed, my results indicate that such firms do exhibit greater asymmetry in their response to ambiguity.

Second, the ambiguity literature suggests a connection between ambiguity and trading volume (Bewley 2002; Dow and Werlang 1992; Epstein and Schneider 2007; Easley and O'Hara 2010). Specifically, ambiguity can be thought of as a lack of information (Camerer and Weber 1992). Since prior research suggests that trading volume is an indication of the presence of information (Beaver 1968), I use trading volume as a proxy for the presence of information to examine whether the asymmetric effects of ambiguity are significantly lower under higher abnormal trading volume. Doing so, I find that the asymmetric responses to bad earnings news are stronger for firms with relatively low abnormal trading volume during the earnings announcement window.

Third, Easley and O'Hara (2010) find that unsophisticated investors are more likely to choose more extreme probability distributions during periods of ambiguity. This finding suggests that asymmetric responses should be more pronounced for firms with a higher proportion of unsophisticated investors. Using institutional ownership percentage as a proxy for a given firm's investor mix, I find that the asymmetric responses to good and bad news are greater for firms with lower institutional stock ownership.

Finally, prior work by Jiang, Lee, and Zhang (2005) and Zhang (2006) investigates the role of information uncertainty in market returns. As a final exploratory analysis, I investigate the cross-sectional relationship between ambiguity and information uncertainty. The results from this analysis show that the asymmetric effect following an increase in the VIX is higher for firms with greater information uncertainty.

This paper makes three primary contributions to the accounting literature. First, this study provides novel evidence that, consistent with theories of ambiguity, a shock to the VIX leads investors to respond asymmetrically to corporate earnings news. While I cannot definitely conclude that ambiguity drives my results, I provide a variety of evidence less consistent with alternative explanations, suggesting that I have isolated a distinct phenomenon concerning investors' responses to information signals in the presence of a macro-level shock. Second, this study contributes to the literature by providing results related to ambiguity in a capital market setting. While ambiguity research dates back to Knight (1921), nearly all of this research is theoretical or lab-based. With the exception of Anderson, Ghysels, and Juergens (2009), empirical studies of ambiguity in actual capital market settings are scarce. Consequently, this study addresses a gap in the literature by using a sample of firms to show how ambiguity affects investor responses to corporate earnings news in a capital market setting. Third, this study contributes to the stream of accounting literature that examines how markets respond to earnings news by providing a novel explanation for documented nonlinearities in the data.

Next, Section II discusses the conceptual framework. Section III discusses the methodology and sample selection. Section IV presents the initial results, robustness tests and subsequent cross-sectional analyses. Section V concludes.

II. CONCEPTUAL FRAMEWORK

Beginning with Ball and Brown (1968) and Beaver (1968), a large body of accounting research investigates how markets respond to earnings news (Kothari 2001). However, prior research offers limited insight concerning how this relationship is affected by macro-uncertainty shocks. In particular, the question of how macro-uncertainty shocks affect the market's response to good versus bad earnings news remains unclear.

Uncertainty shocks provide a unique setting in which to study the market's processing of earnings news. Specifically, an unexpected increase in macro-uncertainty can create significant trepidation among investors about the levels of influential state variables (Bansal and Yaron 2004; Drechsler and Yaron 2011). Moreover, immediately after a shock, investors become particularly sensitive to the possibility of future shocks (Todorov 2010). This sensitivity leads to a lack of confidence due to *ambiguity* concerns with respect to the underlying macro-economic environment and the relation between the macro environment and the firms operating within it.

Knight (1921) is credited as the first to make a distinction between *risk* and *ambiguity*. Specifically, Knight (1921) characterizes risk as scenarios where the decision-maker is uncertain over *outcomes*. *Ambiguity*, on the other hand, occurs when the decision-maker faces uncertainty over *probabilities*. In an early study, Ellsberg (1961) provides evidence that people behave as though they make a distinction between risk and ambiguity. Moreover, Ellsberg (1961) further shows that people are averse to ambiguity. Subsequent research has focused on the application of Knight's (1921) and Ellsberg's (1961) work to study how ambiguity impacts the decision-making process of investors.

In one such study, Epstein and Schneider (2008) model a scenario where investors know that the true precision of an information signal is contained in a set of possible precisions, but cannot assess priors over this set. In this scenario, the wider the range of possible signal precisions in the investor's set, the greater the ambiguity. Following Gilboa and Schmeidler (1989) and Epstein and

Schneider (2003), an ambiguity-averse agent behaves as if he maximizes expected utility under the worst-case belief from the set of possible probabilities. Epstein and Schneider (2008) then show that when an ambiguous signal conveys bad news, the worst case is that the signal is very reliable and the investor responds strongly, and *vice versa* for good signals.[2]

Ambiguity with respect to the precision of earnings releases can potentially result when an economic shock creates a lack of confidence by investors, resulting in a reduction of investor confidence in their *interpretation* of the implications of earnings signals (e.g., signal interpretation *a la* Kim and Verrecchia 1994). While not modeled in Epstein and Schneider (2008),[3] an alternative formulation assumes that an economic shock creates ambiguity with respect to the volatility of fundamentals, leading to similar predictions.[4]

Alternatively, ambiguity can affect investors' processing of firm information in a manner consistent with the following quote from Hansen and Sargent (2010): "A pessimist thinks that good news is temporary but that bad news will endure." Pursuing this motivation, Hansen and Sargent (2010) model a representative consumer who evaluates consumption streams in light of model selection and parameter estimation problems. The consumer is uncertain as to which model governs future consumption growth, where one model exhibits persistence in information shocks and the other does not. The arrival of signals induces the consumer to alter his posterior distribution over models. However, due to specification doubts or ambiguity, the consumer updates his priors over models by slanting the probabilities pessimistically. Thus, negative signals lead the cautious consumer to slant his probability assessments toward the model where such negative shocks are persistent, while good news signals push his assessments toward the model of low persistence. While this is a representative consumer model focused on the macro-economy, the possibility that macro shocks could cause investors to be uncertain about the persistence of current earnings seems at least plausible. This is especially true if a firm's underlying wealth-generating process is highly connected to the macro-economy, as I discuss in Section V. That is, if investors observe a bad-news earnings signal, then they may pessimistically slant toward the worst-case model of high persistence and, thus, react strongly. On the other hand, a good-news signal may slant them toward the belief that this signal will not persist. In this case, they may move toward a model where the persistence of the signal is low and, thus, respond weakly.

Both of the above arguments lead me to predict that in ambiguous environments, investors will respond more strongly to bad earnings news than to good earnings news. Conversely, in non-ambiguous environments, I predict that investors will respond symmetrically to earnings news. Since there are many different channels through which ambiguity could potentially affect investor

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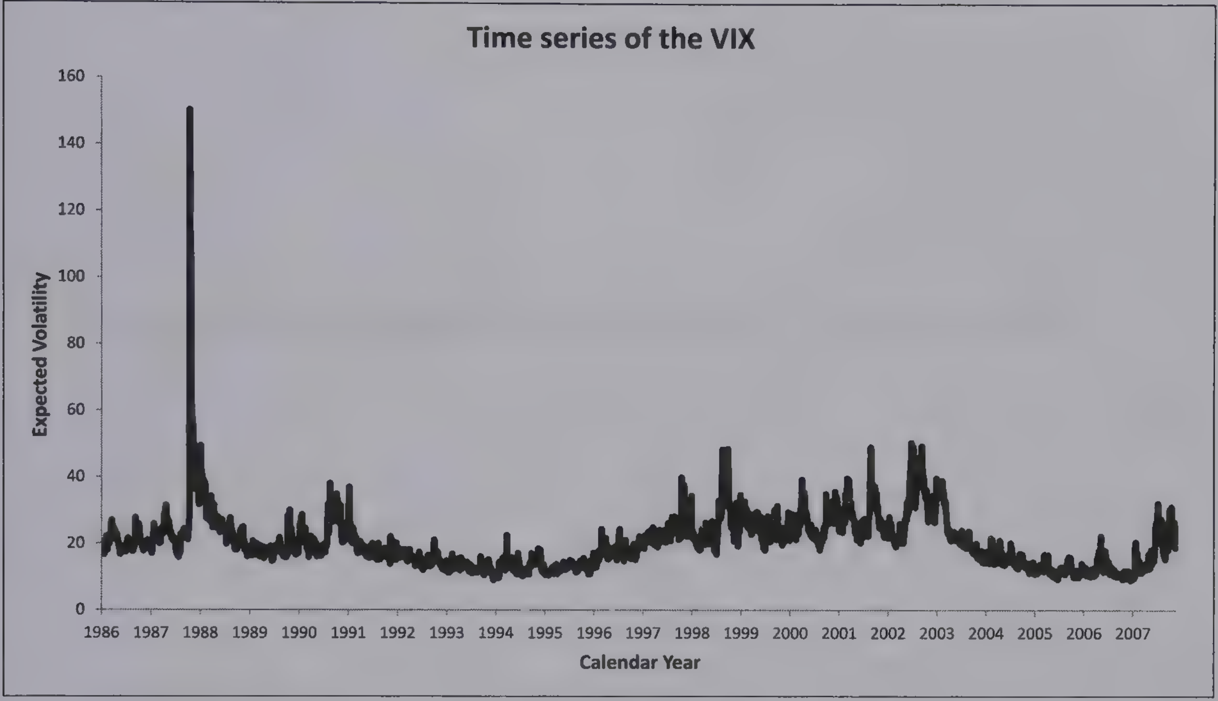
[2] To be more concrete, suppose v is the parameter that investors want to learn; however, these investors can observe only the noisy signal $s = v + \varepsilon$. The noise term is represented by: $\varepsilon \sim N(0, \sigma_\varepsilon^2)$, where $\sigma_\varepsilon^2 \in [\underline{\sigma_\varepsilon^2}, \overline{\sigma_\varepsilon^2}]$. Epstein and Schneider (2008) assume that when decision-makers see that the signal (s) is bad news, the precision $= 1/\underline{\sigma_\varepsilon^2}$. When they observe a good signal, the precision $= 1/\overline{\sigma_\varepsilon^2}$. This behavior results in a stronger response to bad news compared to good news. To see this more clearly, assume $E[s] = 0$, $\mathrm{cov}(v,\varepsilon) = 0$ and note that $E[v|s] = \frac{\mathrm{cov}(v,s)}{\mathrm{var}(s)} * s = \frac{\sigma_v^2}{\sigma_\varepsilon^2 + \sigma_v^2} * s$. If $s < 0$, then the most pessimistic response is achieved by assuming that $\sigma_s^2 = \underline{\sigma_s^2}$, which makes the coefficient on s as high as possible. Conversely, if $s > 0$, then the most pessimistic response is achieved by assuming that $\sigma_s^2 = \overline{\sigma_s^2}$, which makes the coefficient on s as low as possible.

[3] Epstein and Schneider (2008) allow for ambiguity with respect to the mean of the fundamentals, not the volatility. See, also, Caskey (2009) for a discussion of this point.

[4] In terms of the example in footnote 1, let the fundamental $v \sim N(0, \sigma_v^2)$, where $\sigma_v^2 \in [\underline{\sigma_v^2}, \overline{\sigma_v^2}]$. Now, recalling $E[v|s] = \frac{\sigma_v^2}{\sigma_\varepsilon^2 + \sigma_v^2} * s$, if $s < 0$, then the most pessimistic response is achieved by assuming that $\sigma_v^2 = \overline{\sigma_v^2}$, which makes the coefficient on s as high as possible. The assumption is that $\sigma_v^2 = \overline{\sigma_v^2}$, is equivalent to assuming that the signal is very informative with respect to the fundamentals, v (and *vice versa* for $s > 0$, where $\sigma_v^2 = \underline{\sigma_v^2}$). Ambiguity in the variance of fundamentals would also likely have an effect on discount rates. To the extent that average information precision is priced (Lambert, Leuz, and Verrecchia 2007), one would still expect an asymmetric response because the increased precision of bad news would reduce the discount rate and thereby increase the response to bad versus good news.

FIGURE 1
Time-Series of the *VIX* and the Δ*VIX*
(1986–2007)

Panel A: Expected Volatility



(continued on next page)

responses to earnings, including the precision of signal, variance of fundamentals, persistence, etc., I remain agnostic as to the channel through which the ambiguity operates.

III. EMPIRICAL FRAMEWORK

Uncertainty Shocks

Historically, the U.S. financial markets have experienced a number of macro-economic shocks—the Cuban Missile Crisis, Black Monday, the Asian Crises, 9/11, and the most recent Financial Crisis, to name a few. Such macro shocks force investors to make decisions in an unfamiliar environment where ambiguity concerns are likely to be high (Epstein and Schneider 2008; Liu et al. 2005). In one study, Bloom (2009) uses the VIX, the weighted average implied volatility for 30-day options on the S&P 100, to document that macro-level shocks lead to large jumps in market-level implied volatility—a proxy for macro-level uncertainty (see Figure 1).[5] Some research suggests this observed reaction in implied volatility is a result of investors using options to protect against ambiguity, and suggests that the time-variation in ambiguity is strongly reflected in

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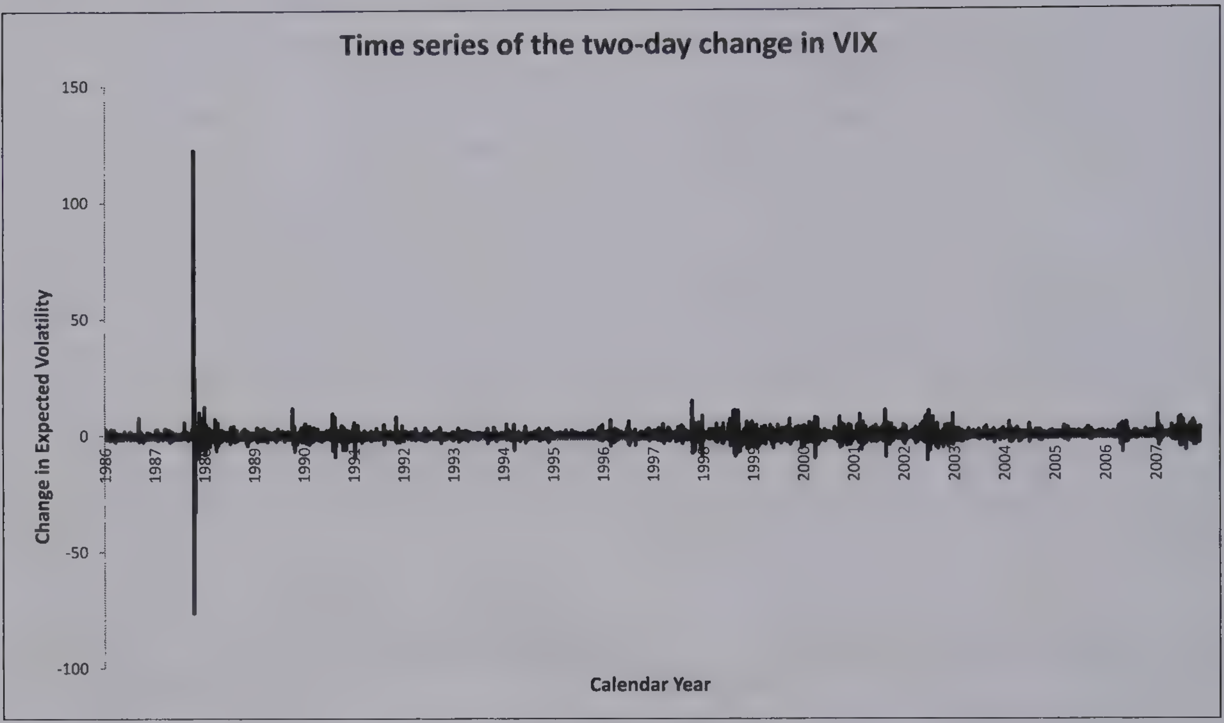
[5] Bloom (2009) also shows that an increase in the VIX is not always accompanied by changes in the level of the market. For example, during the Cuban Missile Crisis, volatility increased, but stock market levels did not change. More recently, Hurricane Katrina resulted in a drop in market levels, but had no impact on volatility.

FIGURE 1 (continued)

Panel B: Change in Expected Volatility



This figure provides the daily time-series of the VIX index for the sample period beginning January 1986 through December 2007. The VIX index data have been collected by the Chicago Board Options Exchange since 1986. The VIX index is constructed by the Chicago Board Options Exchange (CBOE), and represents the implied volatility of the at-the-money option for the S&P 100 index with a maturity of one month. Specifically, it is calculated from eight S&P 100 index calls and puts. It takes into account U.S. features of option contracts, discrete cash dividends, and microstructure frictions such as bid-ask spreads. Panel A presents the level of the index, whereas Panel B shows the times series of the two-day change.

variation in the VIX. For example, Drechsler (2013) models a direct link between time-varying ambiguity and the premiums in index options. In another study, Miao, Wei, and Zhou (2012) find that 96 percent of the observed index option premium is due to ambiguity.[6]

Given this potential link between ambiguity and the VIX, I use the two-day change in the VIX immediately prior to an earnings announcement to capture any uncertainty shocks. This use of a two-day change in the VIX provides two advantages in my study. First, since macro-uncertainty shocks are accompanied by *changes* in the VIX (Bloom 2009), it provides a powerful setting in which to examine the effects of ambiguity because investors are in a new and unfamiliar setting (Heath and Tversky 1991). Moreover, investors have not yet had enough experience in the environment to become comfortable with the relevance of various types of information (Epstein and Schneider 2008) or to learn about the true underlying distributions governing the economy and the firms or the relation between the two (Epstein and Schneider 2007).

The second benefit of using a two-day change in the VIX immediately prior to the earnings announcement window is that it also provides a potentially strong empirical setting in which to rule out alternative explanations for my findings. For example, prior research has consistently suggested a relation between management behavior and uncertainty (e.g., Dye 1985; Verrecchia 1990; Nagar 1999; Houston, Lev, and Tucker 2010; Kim, Pandit, and Wasley 2010). Given this relation, by taking the two-day change immediately preceding the earnings announcement, I select a window where the ability of management to respond to the shock is severely restricted. In doing so, I mitigate potential concerns that my results are driven by managerial incentives to alter the earnings signal in response to the uncertainty shock.

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[6] The index option premium is commonly referred to as the "variance premium." Todorov (2010) shows that variation in the variance premium directly translates into variation in the VIX.

To illustrate changes in the VIX during the period of my study, I sketch out the time-series behavior of both the level of the VIX and the two-day change in the VIX for the period 1986–2007 in Figure 1, Panels A and B. The data in Figure 1 show that the mean and median levels of the VIX are 20.38 and 19.09, with 99th and 1st percentile values of 44.96 and 10.16, respectively. Consistent with findings in previous research, the max value of the VIX within this time frame occurs on Black Monday in October of 1987.[7] The mean (median) change in the VIX over the time period is 0.00 (–0.05). During the same time period, the interquartile range of change in the VIX increases from –0.86 to 0.79, with changes at the 95th (5th) and 99th (1st) percentiles of 2.93 (–2.82) and 6.18 (–5.87), respectively.

One potential limitation of using changes in the VIX to measure uncertainty shocks relates to the magnitude of the change. Specifically, large changes in the VIX are more likely to be associated with ambiguous environments, while small changes are less likely to reflect unfamiliar environments. To address this issue in later analyses, I use ranked changes in the VIX and examine the responses across the ranks. At this point, I want to caveat that while there is some support for a link between the changes in the VIX and ambiguity, there is no standard method of measuring ambiguity (see Hitchens [2014] for an alternative estimation procedure). Because of this, the potential limitation in using a two-day change in the VIX is the level of coarseness in my data, which precludes a direct observation of the investors' distribution set. This limitation may introduce potential noise into my measure. To mitigate this concern, I take three approaches to try to mitigate this concern. First, I use alternative proxies for macro-uncertainty shocks. Second, I try to rule out alternative explanations that are based on either risk or psychological biases. Third, I use cross-sectional splits that are guided by ambiguity theory.

Methodology

To conduct my analyses, I follow the methodology in Conrad et al. (2002) to empirically model differential responses to bad and good earnings news using the following specification:

$$Ret_{it} = \beta_0 + \beta_1 Badnews_{it} + \beta_2 Goodnews_{it} + \varepsilon_{it}, \tag{1}$$

where *Ret* is equal to the three-day (t–1 to t+1) cumulative market-adjusted return for firm i over the quarterly earnings announcement date t. I compute *Badnews* and *Goodnews* by calculating a firm's seasonally adjusted unexpected earnings (*UE*), scaled by average total assets. Note that *Badnews* (*Goodnews*) is equal to *UE* when $UE < (>)\ 0$, and 0 otherwise. Furthermore, coefficient β_1 represents the market's response to bad news, while β_2 represents the market's response to good news. To test for the effects of changes in the VIX, I measure the two-day change in the VIX immediately preceding the announcement window (i.e., $t-1$ to $t+1$). I then partition observations based on increases in the VIX (ΔVIX^+) and decreases in the VIX (ΔVIX^-), estimating Equation (1) within each of the partitions.

To mitigate any potential coefficient bias (Barth and Kallapur 1996), I also include a size variable (*Size*), defined as the natural logarithm of a firm's market value of equity measured at the end of the fiscal quarter. In addition, I include the average level of the VIX over the week prior to the event to control for the magnitude of the VIX effect on firm returns. To control for differences in interim information flows, coefficient bias, and the general information environment over the quarter (Ataise 1985, 1987), I include each firm's return over the prior quarter (Ret_{pre}), as well as the number of analysts following the firm (*#Analysts*). Finally, given the nature of my partitioning variable, I include year fixed effects and cluster the standard errors on both the firm and time dimensions.

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[7] In untabulated results, my results are robust to the exclusion of the Black Monday event from the sample.

Data

To obtain my sample, I first collect firm and market securities data from the CRSP, and financial accounting data and earnings announcement dates from the Compustat quarterly file. To be included in the sample, a firm must have a complete set of financial variables, as well as quarterly announcement dates. In addition to my firm data, I collect analyst data from I/B/E/S and VIX data from the CBOE.

Following Ball, Kothari, and Shanken (1993), I then eliminate those observations with prices less than or equal to $5 prior to the event window, to minimize the effects of market frictions. I also require each firm in my sample to be actively traded over the entire event window. To reduce the possibility that my findings are driven by illiquidity, I follow Chordia and Swaminathan (2000) and eliminate any observations with more than two consecutive days of zero trading in the week prior to the event window. To eliminate the effect of outliers, I also (1) delete firms with negative book value of equity (Barth, Beaver, and Landsman 1998), (2) delete firms with a ratio of earnings-to-market capitalization greater than one on the announcement day (Conrad et al. 2002), and (3) trim all firm-level variables at the 1st and 99th percentiles, with *Badnews* trimmed at the 1st percentile and *Goodnews* trimmed at the 99th percentile (Freeman and Tse 1992). I also require that each firm-quarter observation have the required variables for all analyses. My final sample consists of 50,978 firm-quarter announcements over the period from January 1986 to December 2007.

Table 1 provides the descriptive statistics for the samples of ΔVIX^{+} and ΔVIX^{-}. The statistics show that the mean market-adjusted return is significantly lower by 11 basis points over the three-day window for the ΔVIX^{+} group. This lower return provides preliminary evidence for a stronger negative reaction to bad news, as the distributions for *Badnews* and *Goodnews* are economically and statistically similar across ΔVIX^{+} and ΔVIX^{-}. This similarity in observed distributions is important because prior literature in accounting has documented nonlinearities in the return-earnings relation (e.g., Freeman and Tse 1992). Consequently, similar distributional properties across the ΔVIX partitions in my sample suggest that nonlinearity can be ruled out as a possible explanation of my earnings response results.

Table 1 also shows that, with the exception of *Size, #Analysts,* and firm pre-announcement market beta computed over the prior quarter (β_{Mrkt}^{pre}), the characteristics of the firm-quarter observations across the ΔVIX^{+} partition and ΔVIX^{-} partition are statistically and economically indistinguishable. Furthermore, while *Size, #Analysts,* and β_{Mrkt}^{pre} are statistically different across partitions, the economic magnitude of these differences is small.

IV. RESULTS

Primary Results

Table 2 presents the initial results from specification (1), partitioned by ΔVIX^{+} and ΔVIX^{-}.

The results in the first column of Table 2 show investor responses to earnings following a decrease in the VIX. The reported *Badnews* (0.6580) and *Goodnews* (0.6503) coefficients are both significantly different from zero; however, a Wald test of the coefficients indicates that the difference in coefficients of 0.0077 is not significantly different from zero. These results

TABLE 1
Sample Descriptive Statistics

Variables	ΔVIX^- (n = 27,188) Mean	ΔVIX^- (n = 27,188) Std. Dev.	ΔVIX^+ (n = 23,790) Mean	ΔVIX^+ (n = 23,790) Std. Dev.	Difference
Ret	0.0049	0.0487	0.0038	0.0488	−0.0011**
Badnews	−0.0017	0.0036	−0.0016	0.0036	0.0000
Goodnews	0.0060	0.0078	0.0060	0.0076	0.0000
VIX	20.3811	7.9896	19.3395	6.8652	−1.0416**
Size	7.1053	1.5142	7.0753	1.5221	−0.0300**
Ret_{pre}	0.0105	0.2110	0.0133	0.4363	0.0027
MTB	2.4111	1.5911	2.4341	1.6268	0.0230
#Analysts	7.5831	5.9706	7.4267	5.9740	−0.1564**
AbVol	1.0531	2.2168	1.0489	2.2264	−0.0041
Lev	0.4290	0.1762	0.4305	0.1755	0.0014
β^{pre}_{Mrkt}	0.8503	0.8005	0.8164	0.8221	−0.0340**
Persistence	0.4941	0.4254	0.4967	0.4308	0.0026
Ret_{Mrkt}	0.0102	0.0198	−0.0016	0.0238	−0.0117**

*, ** Indicate significance at the 0.10 and 0.05 levels, respectively.

The ΔVIX partitions ΔVIX^+ (ΔVIX^-) indicates an increase (decrease) in the VIX two days prior to the earnings announcement window.

Variable Definitions:
Ret = three-day (*t*–1 to *t*+1) cumulative market-adjusted return for firm *i* over the quarterly earnings announcement date *t;*
Badnews (*Goodnews*) = *UE* when *UE* < (>) 0, and 0 otherwise;
VIX = average level of the VIX over the week prior to the window in which the change in the VIX is measured;
Size = natural logarithm of the market value of equity;
Ret_{pre} = firm's return over the quarter leading up to the earnings announcement event window;
MTB = market-to-book ratio for the firm;
#Analysts = number of analysts following the firm over the quarter;
AbVol = abnormal turnover for the event window;
Lev = total liabilities to total assets for the quarter;
β^{pre}_{Mrkt} = firm's market beta from a market model over the quarter leading up to the measurement of the change in the VIX;
Persistence = AR(1) coefficient from a seasonally adjusted random walk model of earnings at quarter *t* on quarter *t*–4 earnings over the prior 20 quarters; and
Ret_{Mrkt} = value-weighted market return over the event window.

indicate that, following a decrease in uncertainty, investors respond to good and bad earnings news symmetrically. I interpret this result as being consistent with investors behaving as though the environment immediately following decreases in the uncertainty is one of *risk*. By contrast, the results in the second column indicate investor response to earnings following an increase in the VIX, or in an ambiguous environment. The reported *Badnews* coefficient for the ΔVIX^+ group is 0.9755 and the *Goodnews* coefficient is 0.5801; both coefficients are significantly different from zero at the p = 0.01 level. In addition, the difference of 0.3954 between the two is significant at the p = 0.01 level, indicating that the *Badnews* coefficient is statistically larger than the *Goodnews* coefficient. To examine changes in asymmetry across columns, I use a permutation test to create an empirical distribution of 1,000 observations. The results indicate that the increased difference between bad and good news responses from 0.0077 (ΔVIX^-) to 0.3954 (ΔVIX^+) is statistically significant at the 0.01 level. This finding of an asymmetric response to bad and good news provides evidence consistent with ambiguity theory. That is,

TABLE 2

Investor Response to Earnings Surprises Following Changes in Uncertainty

	Dependent Variable: Ret_{it}	
Variables	ΔVIX^-	ΔVIX^+
Badnews	0.6580***	0.9755***
	(0.0944)	(0.1031)
Goodnews	0.6503***	0.5801***
	(0.0467)	(0.0528)
VIX	−0.0001**	−0.0001*
	(0.0001)	(0.0001)
Size	−0.0013***	−0.0005*
	(0.0003)	(0.0003)
Ret_{pre}	−0.0112***	−0.0033**
	(0.0031)	(0.0017)
#Analysts	0.0002**	0.0003***
	(0.0001)	(0.0001)
n	27,188	23,790
Test of Asymmetry		
Badnews = *Goodnews*	0.0077	0.3954***
Difference in Asymmetry		
$\Delta VIX^- = \Delta VIX^+$	0.3877***	

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*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table 2 presents the results from OLS regressions of returns on quarterly earnings news and with control variables from 1986–2007 estimated by the ΔVIX group, where ΔVIX^+ (ΔVIX^-) indicates an increase (decrease) in the VIX two days prior to the event window. Regressions also include year fixed effects. The significance level is calculated using the error clustered along firm and time.

Variable Definitions:

Ret = three-day (t–1 to t+1) cumulative market-adjusted return for firm i over the quarterly earnings announcement date t;
Badnews (*Goodnews*) = *UE* when *UE* < (>) 0, and 0 otherwise;
VIX = average level of the VIX over the week prior to the window in which the change in the VIX is measured;
Size = natural logarithm of the market value of equity;
Ret_{pre} = firm's return over the quarter leading up to the earnings announcement event window; and
#Analysts = number of analysts following the firm over the quarter.

increases in uncertainty seem to place investors in an environment where their ambiguity concerns lead them to weigh bad and good news differently.

I next dissect the asymmetric result presented in Table 2 to analyze whether the magnitude of the shock is of importance in determining investor responses. While prior research suggests that investors dislike increases in uncertainty, the objective of this paper is to investigate the effect of shocks that place investors in unfamiliar environments. Extending this idea, I would expect that larger observed shocks should generate greater investor ambiguity concerns than smaller ones. To examine this idea, I first partition the changes in uncertainty for the sample into quintiles and then estimate Equation (1) for each quintile.

Table 3 reports the results and Figure 2 provides a graphical representation of the coefficient estimates across the quintiles. Specifically, the results in Table 1 show that the mean percentage change in the VIX for each quintile ranges from −10.9 percent (quintile 1) to 14.0 percent (quintile 5). However, it is only at quintile 5 where there is a statistically significant

TABLE 3

Investor Responses to Earnings Surprises Following Changes in Uncertainty Quintiles

	Δ*VIX* Quintile				
Variables	**1st**	**2nd**	**3rd**	**4th**	**5th**
Badnews	0.5626***	0.7509***	0.7861***	0.9782***	1.0151***
	(0.1509)	(0.1534)	(0.1542)	(0.1590)	(0.1556)
Goodnews	0.5764***	0.7072***	0.6835***	0.6590***	0.5181***
	(0.0737)	(0.0743)	(0.0786)	(0.0755)	(0.0797)
VIX	−0.0002**	−0.0001	−0.0003*	0.0001	−0.0002**
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Size	−0.0027***	−0.0004	−0.0003	−0.0012***	−0.0001
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0005)
Ret_{pre}	−0.0086***	−0.0118**	−0.0244***	−0.0226***	−0.0023*
	(0.0031)	(0.0047)	(0.0047)	(0.0041)	(0.0012)
#Analysts	0.0005***	−0.0001	0.0001	0.0003***	0.0002*
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Mean % Change in VIX	−10.9%	−4.2%	−0.5%	3.3%	14.0%
Test of Asymmetry					
Badnews = *Goodnews*	−0.0138	0.0437	0.1026	0.3192	0.4970***

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table 3 presents the results from OLS regressions of returns on quarterly earnings news and control variables from 1986–2007 estimated by Δ*VIX* quintile. Regressions also include year fixed effects, and the significance level is calculated using the error clustered along firm and time.

Variable Definitions:
Ret = three-day (t−1 to t+1) cumulative market-adjusted return for firm i over the quarterly earnings announcement date t;
Badnews (*Goodnews*) = *UE* when *UE* < (>) 0, and 0 otherwise;
VIX = average level of the VIX over the week prior to the window in which the change in the VIX is measured;
Size = natural logarithm of the market value of equity;
Ret_{pre} = firm's return over the quarter leading up to the earnings announcement event window; and
#Analysts = number of analysts following the firm over the quarter.

asymmetry in investor responses to good and bad news.[8] The findings in Table 3 suggest that investors exhibit ambiguity concerns only after large increases in the VIX, consistent with the conjecture that large shocks are likely to create sufficiently ambiguous environments.

Primary Results Using Alternative Measures of Uncertainty Shocks

The primary analyses in this paper rely on changes in the VIX to capture macro-uncertainty shocks. This section examines the sensitivity of my results to alternative proxies for macro-uncertainty shocks and other proxies for ambiguity. For my first alternative measure of uncertainty shocks, I follow Bloom (2009) and select the following seven events that occur during my sample period and are considered macro-uncertainty shocks. These events are: Black Monday (October 1987); Gulf War I (September–October 1990); the Asian Crisis (November 1997); Russian and Long-Term Capital Management (LTCM) default (September 1998); the 9/11 Terrorist Attack

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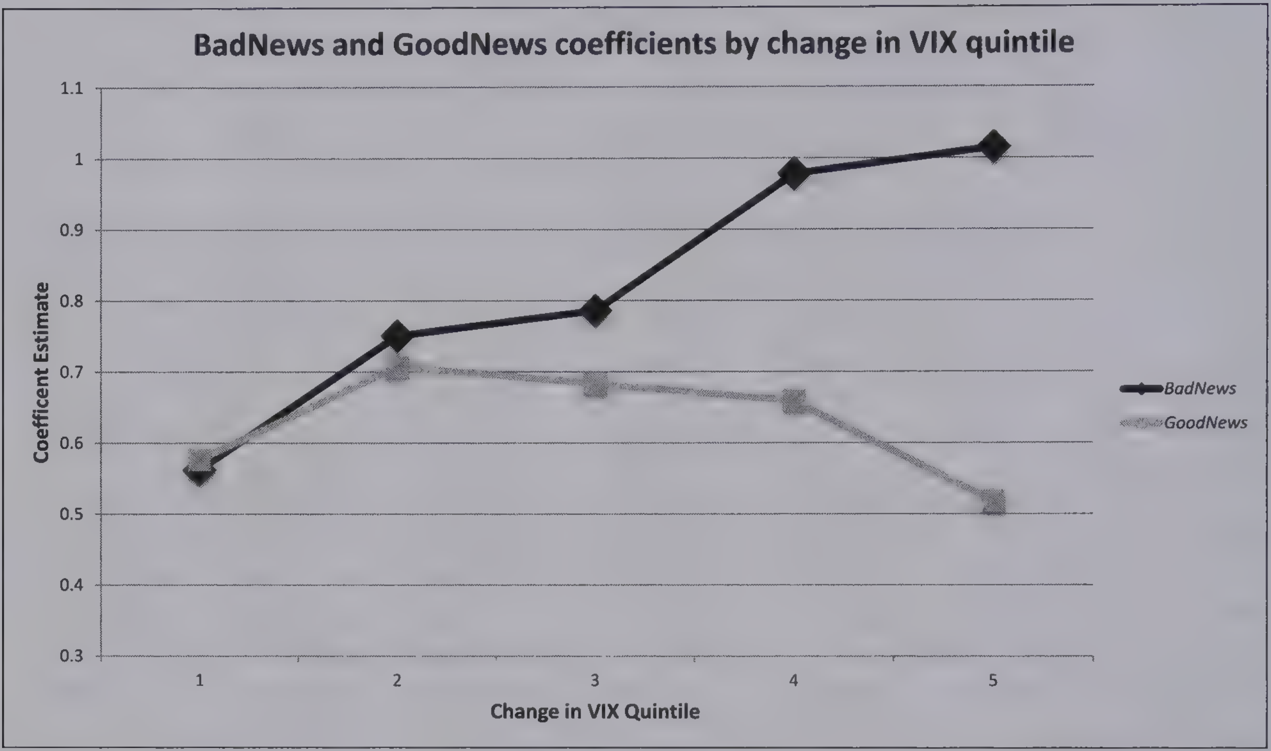
[8] In untabulated results, I partition the data into deciles and find significant asymmetry in only the tenth decile.

FIGURE 2
Plot of *Badnews/Goodnews* Coefficients by Change in VIX (Δ*VIX*) Quintiles



The figure plots the *Badnews* and *Goodnews* coefficients from Table 3.

(September 2001); the WorldCom and Enron Scandals (July–September 2002); and Gulf War II (February 2003). While, ideally, I would like to include only observations immediately following these events, for some of the events, pinpointing an exact day would be difficult. Hence, I use the month(s) in which each event is generally believed to have occurred as the event window. I then test to see if there is an asymmetric response to earnings news in the event window compared to earnings news outside the event window. The strength of this design is that it uses exogenous and unpredictable macro events. However, a weakness of this design is that measuring the exact point of the shock is unclear and, therefore, introduces noise into the estimation. Table 4, Panel A provides the estimation results for my two partitions. Consistent with earlier findings, I find that investors respond to bad and good news asymmetrically around an event; however, outside an event window, the difference in investor reaction is not statistically significant. These results are again consistent with theories of ambiguity.

In addition to looking at the change in the VIX and the occurrence of macro-economic events, I use two alternative proxies to measure macro-uncertainty shocks and ambiguous environments. The first proxy is the variance premium. Drechsler (2013) suggests that the variance premium can be explained with ambiguity theory, which gives it the benefit of being directly linked to theory. However, a weakness of the measure, noted by Drechsler (2013), is that it is highly correlated with the VIX, and so many of the same weaknesses in the VIX apply to the variance premium. In the spirit of Drechsler (2013), I create a rough proxy for the variance premium by taking the difference between the VIX and the realized variance of the index futures

TABLE 4

Investor Reactions and Alternative Proxies for Uncertainty Shocks and Ambiguous Environments

Panel A: Macro-Economic Events

	Dependent Variable: *Ret*	
Variables	**Non-Event**	**Event**
Badnews	0.7798***	1.2531***
	(0.0750)	(0.3861)
Goodnews	0.6300***	0.4691**
	(0.0372)	(0.2070)
Controls	Included	Included
n	49,194	1,748
Test of Asymmetry		
Badnews = *Goodnews*	0.1498	0.7840**

Panel B: Change in the Variance Premium

	Dependent Variable: *Ret*	
Variables	ΔVP^{-}	ΔVP^{+}
Badnews	0.8353***	0.7991***
	(0.0988)	(0.0979)
Goodnews	0.7434***	0.04945**
	(0.0487)	(0.0501)
Controls	Included	Included
n	26,702	24,276
Test of Asymmetry		
Badnews = *Goodnews*	0.1498	0.3046**

Panel C: Level of the Variance Premium

	Dependent Variable: *Ret*	
Variables	VP^{Low}	VP^{High}
Badnews	0.7684***	0.8354***
	(0.1006)	(0.1025)
Goodnews	0.6634***	0.5757***
	(0.0488)	(0.0495)
Controls	Included	Included
n	27,188	23,790
Test of Asymmetry		
Badnews = *Goodnews*	0.1050	0.2597**

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*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table 4 presents the results from OLS regressions of returns on quarterly earnings news and control variables from 1986–2007 estimated by the uncertainty shock events. Earnings announcements that take place during the month of the uncertainty shock event are coded as an *Event;* all other announcements are coded as *Non-Event*. Regressions also include year fixed effects, and the significance level is calculated using the error clustered along firm and time.

(continued on next page)

TABLE 4 (continued)

Variable Definitions:
VP = difference between the VIX and the subsequent squared realized market return over the subsequent 30 days;
ΔVP^- (ΔVP^+) = an indicator variable equal to 0 (1) if the two-day change in VP is negative (positive);
VP^{Low} (VP^{High}) = an indicator variable equal to 0 (1) if VP is below (above) the median;
Ret = three-day (t–1 to t+1) cumulative market-adjusted return for firm i over the quarterly earnings announcement date t;
$Badnews$ ($Goodnews$) = UE when $UE < (>) 0$, and 0 otherwise;
$Size$ = natural logarithm of the market value of equity;
Ret_{pre} = firm's return over the quarter leading up to the earnings announcement event window; and
$\#Analysts$ = number of analysts following the firm over the quarter.

using squared daily returns on the futures.[9] I then use both the level of the premium (VP) and the change in the premium (ΔVP) over the same window as I compute the change in the VIX. For the level of VP, I partition the data at the median and reestimate Equation (1) for the VP^{Low} and VP^{High}. Similar to ΔVIX, I partition ΔVP into increases (ΔVP^+) and decreases (ΔVP^-). To the extent that high levels of VP and shocks in VP (i.e., ΔVP^+) capture ambiguous environments, investors should respond asymmetrically to earnings news. The results of these alternative measures of ambiguity, reported in Table 4, Panels B and C, provide evidence consistent with my predictions.

Overall, the evidence in Tables 2, 3, and 4 is consistent with the prediction that uncertainty shocks, as measured by increases in the VIX or macro events, create an ambiguous environment that induces ambiguity concerns for investors as they attempt to process firm-specific earnings information. This leads investors to react more strongly to bad versus good news, especially in the context of a large shock.

Alternative Explanations

Alternative Explanation—Leverage and Volatility Feedback Effects

Prior research has documented that market-level volatility is associated with skewness in market- and firm-level returns, referred to in the literature as the asymmetric volatility phenomenon (Black 1976; Christie 1982). The literature attributes the asymmetric volatility relation to leverage (Black 1976; Christie 1982; Schwert 1989), volatility feedback (French, Schwert, and Stambaugh 1987; Campbell and Hentschel 1992), or a combination of the two (Bekaert and Wu 2000; Wu 2001). In the following analysis, I investigate each driver in turn.

First, the observed asymmetric response may be due to leverage effects. In this case, bad news can increase firm leverage and, thus, the firm's idiosyncratic risk. While standard asset pricing theory suggests idiosyncratic risk should not be priced, research results suggest otherwise (Goyal and Santa-Clara 2003). To control for any potential leverage effect, I include leverage in the regression, interacting it with the *Badnews* and *Goodnews* variables.

I further draw on the work of Bekaert and Wu (2000), who provide a unified framework that considers both the leverage and volatility feedback effects at the firm level. Their study shows that market-level bad (good) news leads to a larger (smaller) negative correlation between contemporaneous returns and conditional volatility at the firm level. To control for market-level news, I include the contemporaneous market return in my analysis. The second component is the firm's covariance. Linking market-level shocks to firm-level returns in

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[9] Prior research uses intraday data to compute the realized variance on the futures. The daily data are coarser and, thus, reduce the power of my measure; however, it is not clear that intraday data would bias in my favor.

TABLE 5

Changing Covariance over the Event Window

	Event	ΔVIX^+			ΔVIX^-			*Badnews*	*Goodnews*
	Day	***Badnews***	***Goodnews***	**Difference**	***Badnews***	***Goodnews***	**Difference**	**Difference**	**Difference**
ΔVIX	−5	0.9929	0.9808	0.0120	0.9184	0.9672	−0.0488*	0.0745*	0.0136
	−4	0.9674	1.0280	−0.0606	1.0053	1.0092	−0.0039	−0.0379	0.0188
	−3	1.0416	1.0346	0.0070	0.9703	0.9814	−0.0110	0.0713*	0.0532*
	−2	0.9497	1.0466	−0.0969**	1.0308	0.9478	0.0830**	−0.0811*	0.0988**
Earnings Window	−1	1.0570	1.0950	−0.0380	0.9539	0.9725	−0.0186	0.1031**	0.1225**
	0	1.0307	1.0698	−0.0391	1.0308	0.9899	0.0409	−0.0001	0.0799*
	1	0.9449	1.0764	−0.1315**	0.9668	0.9624	0.0044	−0.0219	0.1140**
	2	0.9666	0.9898	−0.0232	0.9590	0.9957	−0.0367	0.0076	−0.0059
	3	0.9531	1.0644	−0.1113**	0.9290	0.9582	−0.0292	0.0241	0.1062**
	4	0.8930	0.9776	−0.0846**	0.8839	0.9485	−0.0646*	0.0091	0.0291
	5	0.9121	0.9596	−0.0475	0.9211	0.9527	−0.0316	−0.0090	0.0069
Pre (−5 to −2)		0.9862	1.0194	−0.0332*	0.9721	0.9813	−0.0092	0.0141	0.0381*
Event (−1 to 1)		1.0175	1.0802	−0.0627**	0.9844	0.9753	0.0091	0.0331	0.1049**
Post (2 to 5)		0.9332	1.0049	−0.0717**	0.9235	0.9646	−0.0411**	0.0097	0.0403**

*, ** Indicate statistical significance at the 0.10 and 0.05 levels, respectively.

The table reports Ball and Kothari (1991) betas estimated by event day around the earnings announcement event window; these are estimated by regressing the firm's daily return on the Fama-French three factors and momentum. The cross-sectional beta is the coefficient on the market portfolio.

standard asset pricing depends on increased covariance between the firm and the market. I explore this possibility by examining whether a macro shock changes the covariance between the firm and market returns.

Ball and Kothari (1991) document that firm betas vary around earnings announcements. Following Ball and Kothari (1991), I estimate event time market betas around the respective earnings announcements for four distinct groups: ΔVIX^+—good news and bad news; and ΔVIX^-—good news and bad news. I estimate the market model controlling for the Fama-French factors and momentum as:

$$(Ret - rf) = \alpha_0 + \beta_{Mrkt}(Mrkt - rf) + \beta_{SMB}SMB + \beta_{HML}HML + \beta_{UMD}UMD + \varepsilon,$$

where *Ret* is the firm return; *rf* is the risk-free rate; *Mrkt* is the market return; *SMB* is the size factor; *HML* is the book-to-market factor; and *UMD* is the momentum factor.[10] Table 5 reports the event time market betas for my sample, where 0 represents the earnings announcement date. My analysis highlights both the period over which I calculate the returns for the earnings announcement ($t-1$ to $t+1$) and the period over which I calculate the change in VIX (close on $t-4$ to close on $t-2$).

In Table 5, for the ΔVIX^- group, I find no noticeable trend in the betas around the earnings announcements for either bad or good news. Furthermore, I find no significant difference across the groups over the majority of the days across the period. For the ΔVIX^+ group, the estimated betas for the bad and good news are generally not different; however, for the good news, there is a slight upward trend in the betas leading up to the announcement. To further analyze this trend, I divide the event days into three subperiods: pre-($t-2$ to $t-5$), event ($t-1$ to $t+1$), and post-($t+2$ to $t+5$). The results across these subperiods, presented at the bottom of Table 5, show no trend in the betas for the ΔVIX^- group; however, for the ΔVIX^+ group, the betas for both groups seem to increase during the event window. Overall, the results of the beta analysis suggest that betas vary around earnings announcements that are preceded by an uncertainty shock.

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[10] I obtained the Fama-French variables from Kenneth French's website at: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/

Finally, the results presented in the last column in Table 5 suggest that changes in the VIX have an effect on market betas for good-news firms. Moreover, in the ΔVIX^+ group, the good-news beta is statistically larger than the bad-news beta on day $t+1$, which occurs within the event window. Prior research has shown that the mapping of earnings surprises into current returns is a function of the market beta (Easton and Zmijewski 1989). Consequently, the above result suggests the increase in the VIX may be driving the observed asymmetry. To control for this possibility, I estimate a firm-specific beta over the window $t-2$ to $t+10$ and interact it with both the *Badnews* and *Goodnews* variables.

To control for the above variables, I use the following modified empirical design:

$$\begin{aligned} Ret = {} & \beta_0 + \beta_1 \Delta VIX + \beta_2 Badnews + \beta_3 Goodnews + \beta_4 \Delta VIX * Badnews \\ & + \beta_5 \Delta VIX * Goodnews + \beta_6 Alternative + \beta_7 Alternative * Badnews \\ & + \beta_8 Alternative * Goodnews + \varepsilon, \end{aligned} \tag{2}$$

where *Badnews* and *Goodnews* are defined previously and ΔVIX is an indicator variable coded as 1 (0) for an increase (decrease) in the VIX. Depending on the alternative explanation proxy, *Alternative* will vary; however, to avoid interacting two continuous variables, I convert all alternative proxies into indicator variables partitioned above (1) and below (0) the median. To test for changes in VIX-induced asymmetry, I examine whether $\beta_4 > 0$ and $\beta_5 \leq 0$, as well as whether the total coefficients are different: $(\beta_2 + \beta_4) > (\beta_3 + \beta_5)$.

Table 6 reports the results after controlling for contemporaneous market returns (Ret_{Mrkt}); firm leverage (*Lev*), defined as total debt over total assets; and the firm's beta post-change in the VIX (β_{Mrkt}). The results in Table 6 show that $\beta_4 > 0$ and $\beta_5 = 0$. The results also indicate that $(\beta_2 + \beta_4) = 1.0644$ and $(\beta_3 + \beta_5) = 0.4323$; the difference of 0.6321 is statistically different from zero (p-value < 0.01). Overall, the findings suggest that the observed asymmetric response by investors to bad and good news following an increase in the VIX is robust to the inclusion of leverage, volatility feedback, and differences in firm market beta controls.

Alternative Explanation—State Risk

Another explanation for the documented asymmetric investor response to earnings news may be that changes in the VIX capture *State Risk,* or the first moment of the market, i.e., good (bad) times or *State Risk.* Conrad et al. (2002) empirically document an asymmetric response to bad and good earnings news. Specifically, they show that in good times, investor stock returns are more sensitive to bad earnings news than good earnings news. Moreover, their empirical results suggest that in bad times, investors respond more symmetrically to good and bad earnings news relative to the good times.

From a conceptual standpoint, in order for the Conrad et al. (2002) effect to explain the observed asymmetry in my results, an increase in the VIX would need to place the market in a good state. However, as noted above, prior research has documented that large increases in the VIX are usually associated with events that are inconsistent with this condition. Moreover, since the VIX is informally known as the "Fear Index," it is unlikely that increases in the VIX would lead to a good market state.

TABLE 6

Alternative Explanations
Investor Responses to Earnings News Following a Change in Uncertainty and Controlling for Leverage and Feedback Effects

Variables	**Dependent Variable: *Ret***
Badnews	0.7783***
	(0.1527)
Goodnews	0.4584***
	(0.0858)
ΔVIX	−0.0004
	(0.0008)
$Badnews * \Delta VIX$	0.2842**
	(0.1419)
$Goodnews * \Delta VIX$	−0.0261
	(0.0759)
Ret_{Mrkt}	−0.0010
	(0.0008)
$Badnews * Ret_{Mrkt}$	−0.2509*
	(0.1418)
$Goodnews * Ret_{Mrkt}$	0.2161***
	(0.0760)
Lev	−0.0024***
	(0.0006)
$Badnews * Lev$	0.0005
	(0.1343)
$Goodnews * Lev$	0.1455**
	(0.0677)
β_{Mrkt}	0.0009
	(0.0008)
$Badnews * \beta_{Mrkt}$	0.0067
	(0.1349)
$Goodnews * \beta_{Mrkt}$	−0.0191
	(0.0704)
Controls	Included
n	50,959
Test of Asymmetry	
$Badnews + (Badnews * \Delta VIX)$	1.0644
$Goodnews + (Goodnews * \Delta VIX)$	0.4323
Difference	0.6321***

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*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table 6 presents the results from OLS regressions of returns on quarterly earnings news and control variables from 1986–2007. Regressions also include year fixed effects, and the significance level is calculated using the error clustered along firm and time.

Variable Definitions:
Ret = three-day (t−1 to t+1) cumulative market adjusted return for firm i over the quarterly earnings announcement date t;
Badnews (*Goodnews*) = *UE* when $UE < (>) 0$, and 0 otherwise;
ΔVIX^{+} (ΔVIX^{-}) = an indicator variable equal to 1 (0) for an increase (decrease) in the VIX two days prior to the event window;
VIX = average level of the VIX over the week prior to the window in which the change in the VIX is measured;
Size = natural logarithm of the market value of equity;

(continued on next page)

TABLE 6 (continued)

Ret_{pre} = firm's return over the quarter leading up to the earnings announcement event window;
#Analysts = number of analysts following the firm over the quarter;
Lev = total liabilities to total assets for the quarter;
β_{Mrkt} = firm's market beta from a Fama-French three-factor model plus momentum estimated from t–2 to t+10; and
Ret_{Mrkt} = value-weighted market return over the event window.

To control for the first-moment effect found in Conrad et al. (2002), I reconstruct Conrad et al.'s (2002) state variable and then include it in specification (2). Because their variable is based on price/earnings (P/E) ratios, I use the last available quarterly earnings number for month t and the shares outstanding as of month t to construct the earnings per share for each firm in each month t. I then compute the market P/E as follows:

$$Market\frac{P}{E_t} = \frac{1}{\left[\sum_{i=1,Nt} w_{it}\left(\frac{EPS_{it}}{price_{it}}\right)\right]},$$

where w_{it} is the value of firm i relative to the total market value of all firms in sample month t. Finally, I compute the moving 12-month average of *Market P/E* and take the difference between each month's *Market P/E* and the 12-month moving average to obtain the difference, *DiffPE*. A high (low) *DiffPE* is classified by Conrad et al. (2002) as an indication of a good (bad) state. I include this variable in Equation (2) as a dichotomous variable, where values greater (less than) 0 are coded 1 (0).

The first column in Table 7 shows that by including *DiffPE*, the interaction $Badnews * \Delta VIX$ is positive and significant (coefficient = 0.3362, p-value < 0.01); however, the interaction on *Goodnews* is insignificant. The results also show that the total *Badnews* coefficient (0.8493) is statistically larger (p-value < 0.01) than the *Goodnews* coefficient (0.6332). These results suggest that my observed ambiguity-induced asymmetric response to good and bad news is robust to the Conrad et al. (2002) effect.

Alternative Explanation—Investor Sentiment

Another potential explanation for my findings is that investor sentiment may create an over- or under-reaction to new information.[11] Indeed, using an index comprised of various sentiment proxies, Baker and Wurgler (2006) find that high investor sentiment leads to overpriced assets, while low investor sentiment leads to underpriced assets. Using Baker and Wurgler's (2006) index, Mian and Sankaraguruswamy (2012) show that high (low) investor sentiment is associated with greater (weaker) investor responses to good versus bad news. In a related study, Livnat and Petrovits (2009) find that holding shares in firms with extreme good news during a pessimistic sentiment period earns higher excess returns than holding good news firms in an optimistic sentiment period. Arif and Lee (2014) show that following periods of high investor sentiment, returns around an earnings announcement are negatively skewed.

Unlike the asymmetry induced by ambiguity, investor sentiment asymmetry occurs across both optimistic and pessimistic periods. This contrasts with the max-min framework, where bad news is weighted more than good news only following increases in the VIX. To control for investor

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[11] Recently, Caskey (2009) uses an ambiguity framework to explain the over- and under-reaction phenomenon as a function of ambiguity-averse investors preferring aggregated information to disaggregated information.

TABLE 7

Alternative Explanations
Investor Responses to Earnings News Following a Change in Uncertainty Controlling for "State Risk," Sentiment, and Book-to-Market

Variables	Dependent Variable: *Ret*		
	State Risk	*Sentiment*	*BTM*
Badnews	0.5131***	0.6021***	0.7448***
	(0.1112)	(0.0957)	(0.1116)
Goodnews	0.7049***	0.6677***	0.7174***
	(0.0607)	(0.0487)	(0.0657)
ΔVIX	−0.0001	−0.0001	−0.0001
	(0.0006)	(0.0006)	(0.0006)
$Badnews * \Delta VIX$	0.3362***	0.4032***	0.3415***
	(0.1325)	(0.1358)	(0.1322)
$Goodnews * \Delta VIX$	−0.0717	−0.1182**	−0.0719
	(0.0675)	(0.0710)	(0.0675)
VIX	−0.0001**	−0.0001**	−0.0001**
	(0.0001)	(0.0001)	(0.0001)
Size	−0.0010***	−0.0009***	−0.0008***
	(0.0002)	(0.0002)	(0.0002)
Ret_{pre}	−0.0050**	−0.0047**	−0.0048**
	(0.0022)	(0.0021)	(0.0022)
#Analysts	0.0002***	0.0002***	0.0002***
	(0.0001)	(0.0001)	(0.0001)
Alternative	0.0001	0.0011	−0.0025***
	(0.0009)	(0.0008)	(0.0006)
$Badnews * Alternative$	0.2553*	−0.0764	−0.2439*
	(0.1372)	(0.1241)	(0.1300)
$Goodnews * Alternative$	−0.0979	−0.2046***	0.0634
	(0.0698)	(0.0597)	(0.0723)
n	50,978	46,832	50,978
Test of Asymmetry			
$Badnews + (Badnews * \Delta VIX)$	0.8493	1.0053	1.0863
$Goodnews + (Goodnews * \Delta VIX)$	0.6332	0.5495	0.6455
Difference	0.2161***	0.4558***	0.4408***

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table 7 presents the results from OLS regressions of returns on quarterly earnings news and control variables from 1986–2007. Regressions also include year fixed effects, and the significance level is calculated using the error clustered along firm and time.

Variable Definition:

Ret = three-day ($t-1$ to $t+1$) cumulative market-adjusted return for firm *i* over the quarterly earnings announcement date *t;*
Badnews (*Goodnews*) = *UE* when *UE* < (>) 0, and 0 otherwise;
ΔVIX^+ (ΔVIX^-) = an indicator variable equal to 1 (0) for an increase (decrease) in the VIX two days prior to the event window;
VIX = average level of the VIX over the week prior to the window in which the change in the VIX is measured;
Size = natural logarithm of the market value of equity;
Ret_{pre} = firm's return over the quarter leading up to the earnings announcement event window;
#Analysts = number of analysts following the firm over the quarter;
State Risk or *DiffPE* = an indicator variable set to 1 (0) if the difference between the current market PE ratio and the average over the prior 12 months is positive (negative);
Sentiment = an indicator variable set to 1 if the Baker and Wurgler (2006) sentiment index is above the median, and 0 otherwise; and
BTM = an indicator variable set to 1 if the firm's book-to-market ratio is above the median, and 0 otherwise.

sentiment, I reestimate Equation (2) and include the Baker and Wurgler (2006) index (*BW_Index*) measured in the month of the announcement and interact it with both *Badnews* and *Goodnews*.[12] The second column in Table 7 reports the results from the inclusion of investor sentiment in the regression. As seen in Table 7, the results for the change in the VIX are robust to the inclusion of investor sentiment.

Alternative Explanation—Market-to-Book (Torpedo Effect)

Skinner and Sloan (2002) document that high market-to-book firms experience relatively more extreme responses to bad earnings news than do low market-to-book firms. This empirical finding, the "torpedo effect," has been attributed to overoptimistic expectational errors (Lakonishok et al. 1994) that are subsequently corrected upon the earnings announcement. If the torpedo effect were responsible for my ambiguity results, then increases in the VIX would need to lead to across-the-board increases in the market-to-book ratio (*MTB*). However, as shown in Table 1, the *MTB* for both increases and decreases in the VIX are statistically equal. To further rule out the torpedo effect, I estimate Equation (2) controlling for the market-to-book ratio. The results, shown in the third column in Table 7, are robust to the inclusion of this variable.

Alternative Explanation—Negativity Bias

Beginning with the work of Kanouse and Hanson (1971) and Peeters (1971), a large literature in psychology documents a negativity bias in individuals, as reviewed by Baumeister et al. (2001) and Rozin and Royzman (2001). The basic notion behind negativity bias is that individuals react more strongly to negative stimuli than to positive stimuli (Akhtar et al. 2011). While the relation between negativity bias and ambiguity aversion has not been studied directly, an examination of the literature suggests the two constructs, or behavioral characteristics, share similar attributes. For example, at a neurological level, psychology research has shown that one of the areas of the brain that is associated with negativity bias is the amygdala, the part of the brain associated with fear and survival instincts (Williams et al. 2009). Similarly, in the neuroeconomics literature, Hsu et al. (2005) and Smith et al. (2002) find that the amygdala is the primary part of the brain that is active when subjects must make decisions in ambiguous settings.

Given these findings, it is possible that my documented asymmetric response to bad versus good news could be attributed to negativity bias. While ambiguity aversion and negativity bias share similar characteristics, they differ in one important aspect. Specifically, negativity bias is seen as *unconditional,* meaning that individuals react to all bad stimuli more strongly than they do to any positive stimuli (Akhtar et al. 2011). On the other hand, the asymmetric response from ambiguity aversion is *conditional* in that only in the presence of ambiguity is there an asymmetric response to the type of news. By extension, if negativity bias is the primary driving force behind my results, then investors should respond asymmetrically to good and bad news regardless of the macro environment or change in the VIX. However, I find that reactions to news type are symmetric following a decrease in the VIX, which is more consistent with theories of ambiguity than with the negativity bias hypothesis.

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[12] This index is based on six measures of investor sentiment: NYSE share turnover, number of IPOs, closed-end fund discount, first-day returns on IPOs, the dividend premium, and the share of equity issues in the total debt and equity issues. To control for business cycles, Baker and Wurgler (2006) regress each of the six measures on growth in the industrial production index, consumer durables, consumer nondurables, and consumer services. After running the first-stage regression, the index is computed as the first principle component of the residuals from the first-stage regression. As before, I construct an indicator variable based on the above and below median *BW_Index* and interact it with *Badnews* and *Goodnews*. Because the index extends only through 2005, my sample is limited to a shorter time period.

Additional Robustness Tests

Prior research has shown that investor responses to earnings announcements vary with the persistence of a firm's earnings (Easton and Zmijewski 1989; Kormendi and Lipe 1987). To address this possibility, I use 20 quarters of data to compute the persistence of a firm's earnings stream, assuming an AR(1) (autoregressive) process (seasonally adjusted), and include the firm's coefficient in Equation (2). I also include the contemporaneous risk-free rate, as this has been shown to impact investor earnings responses (Collins and Kothari 1989). Doing so, I find that my results (not tabulated) remain unchanged.

As stated above, my primary interest is in understanding how macro-uncertainty shocks affect investors' processing of information; however, it could be argued that the level of macro-uncertainty might also be a good proxy for ambiguity and, therefore, might affect investor processing of firm-specific information. To examine this possibility, I partition the data according to the median level of the VIX on the day prior to the earnings announcement, and I then reestimate Equation (1). The results (not reported) show that in the high (low) VIX partition, investors respond asymmetrically (symmetrically) to bad and good news.

Finally, I perform three additional robustness analyses to rule out the possibility that my documented asymmetric response is due to short-selling constraints or limits to arbitrage, financing constraints, or expected changes in firm investments. First, to control for short-selling constraints, I interact the bad and good news variables with an indicator variable equal to 1 if the firm has traded put options, and 0 otherwise.[13] To control for financing frictions, I interact the bad and good news variables with firm size (Hadlock and Pierce 2010). To control for expected changes in firm investment, I interact the bad and good news variables with a firm's capital expenditures over the subsequent 12 months (Bloom 2009). I find that my results (not reported) are robust to the addition of these control variables.

To summarize, I investigate multiple alternative explanations for my documented asymmetric investor response to bad versus good news, and find that the asymmetric weighting of news type following an increase in the VIX or an uncertainty shock is robust to these alternative explanations. Consequently, I conclude that the observed asymmetry is likely due to investor responses to ambiguity.

Cross-Sectional Tests

Ambiguity Susceptibility

To gain better insight into my findings, I next explore the role of firm characteristics in determining a firm's susceptibility to ambiguity. Specifically, I consider the question of whether some firms have a greater potential to experience the effects of uncertainty shocks than others because of their underlying wealth-generating process and its connection to the macro-economy. This intuition is based on the relation between the VIX and macro or general ambiguity. Given this relation, it is possible that the effects of ambiguity would be more pronounced for firms whose underlying earnings process is highly tied to the macro environment. To investigate this possibility, I test for cross-sectional variation in market responses to earnings information based on a firm's return covariation with changes in the VIX (Ang, Hodrick, Xing, and Zhang 2006).

Following Ang et al. (2006), I use daily data obtained from the CRSP daily database over a 20-day period ending ten days prior to the event window to estimate the following two-factor model:

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[13] Blau and Brough (2014) provide evidence that firms with high shorting activity are likely to be listed on option markets. However, to the extent that there are firms that have active shorting markets, but no traded put options, this may introduce noise into my measure. The "Investor Sophistication" section below also provides some evidence that is inconsistent with a limit to arbitrage argument.

TABLE 8

Ambiguity Susceptibility and Investor Asymmetric Responses to Earnings Surprises Following Changes in Uncertainty: $\beta_{\Delta VIX}$

	Dependent Variable: *Ret*			
	ΔVIX^-		ΔVIX^+	
Variables	$\beta^{Low}_{\Delta VIX}$	$\beta^{High}_{\Delta VIX}$	$\beta^{Low}_{\Delta VIX}$	$\beta^{High}_{\Delta VIX}$
Badnews	0.6698***	0.6424***	0.9467***	1.0406***
	(0.1314)	(0.1364)	(0.1496)	(0.1442)
Goodnews	0.6492***	0.6539***	0.6167***	0.5698***
	(0.0661)	(0.0661)	(0.0737)	(0.0740)
VIX	−0.0002*	−0.0001	−0.0001	−0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Size	−0.0016***	−0.0011***	−0.0009**	−0.0002
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Ret_{pre}	−0.0111**	−0.0112***	−0.0184***	−0.0023*
	(0.0045)	(0.0041)	(0.0041)	(0.0012)
#Analysts	0.0003***	0.0001	0.0004***	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
n	13,609	13,579	11,858	11,932
Test of Asymmetry				
Badnews = *Goodnews*	0.0206	−0.0115	0.3300*	0.4708***

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*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table 8 presents the results from OLS regressions of returns on quarterly earnings news and control variables from 1986–2007 estimated by *ΔVIX* and $\beta_{\Delta VIX}$. Regressions also include year fixed effects, and the significance level is calculated using the error clustered along firm and time.

Variable Definitions:
ΔVIX^+ (ΔVIX^-) = an increase (decrease) in the VIX two days prior to the event window;
$\beta_{\Delta VIX}$ = coefficient from the estimation of returns on the market portfolio and the daily change in the VIX (Ang et al. 2006);
Ret = three-day (t−1 to t+1) cumulative market-adjusted return for firm i over the quarterly earnings announcement date t;
Badnews (*Goodnews*) = *UE* when *UE* < (>) 0, and 0 otherwise;
VIX = average level of the VIX over the week prior to the window in which the change in the VIX is measured;
Size = natural logarithm of the market value of equity;
Ret_{pre} = firm's return over the quarter leading up to the earnings announcement event window; and
#Analysts = number of analysts following the firm over the quarter.

$$(Ret - rf) = \alpha_0 + \beta_{Mrkt}(Mrkt - rf) + \beta_{\Delta VIX}\Delta DVIX + \varepsilon,$$

where $\Delta DVIX$ is the one-day change in the VIX. In this model, $\beta_{\Delta VIX}$ can be interpreted as a measure of a firm's sensitivity or susceptibility to the effects of uncertainty shocks. Specifically, I conjecture that firms whose underlying earnings process has a large systematic component will be firms with a high $\beta_{\Delta VIX}$ and will also exhibit stronger responses to uncertainty shocks. On the other hand, decreases in the VIX should create no differential effects across firms.

The results of this analysis, presented in Table 8, show my estimation of Equation (1) for high/low ($\beta^{High}_{\Delta VIX}/\beta^{Low}_{\Delta VIX}$) VIX beta groups. These results indicate that, following an increase in the VIX, firms in the high VIX beta group exhibit strong asymmetry, while those in the low group exhibit marginal asymmetry. Furthermore, the results show that for both the high and low VIX beta groups, responses to bad and good news are symmetric following a decrease in the VIX. These results are consistent with my conjecture; the asymmetric response to bad news relative to good news following an increase in the VIX is stronger for the high $\beta_{\Delta VIX}$ (i.e., $\beta_{\Delta VIX}^{High}$).

Trading Volume

The crux of the ambiguity hypothesis is that ambiguity-averse investors lack the relevant information to form unique priors in an ambiguous environment. This section explores this idea more fully by examining whether a lack of information exacerbates the asymmetric effects of ambiguity. To test this conjecture, I use trading volume during the earnings announcement event window as a proxy for the amount of information in the environment. Prior research has shown that trading volume is associated with information arrival (Beaver 1968) and that it is related to the level of ambiguity in the environment (Dow and Werlang 1992; Epstein and Schneider 2007).[14]

In their study, Dow and Werlang (1992) use a max-min framework to examine how ambiguity affects investor decisions on whether to participate in the market. Specifically, they show that a lack of information (the presence of ambiguity) about the correct data-generating process creates a wedge between the price agents are willing to pay to go long in an asset (ask price) and the price agents are willing to pay to go short (bid price). This increased spread, in turn, reduces the incentive for ambiguity-averse agents to participate in the market.

Epstein and Schneider (2007) extend the study of Dow and Werlang (1992) to an intertemporal setting and find that an increase in ambiguity reduces market participation by ambiguity-averse investors. However, they find that this investor reluctance reverses as the ambiguity shock is resolved. This implies that the observed behavior of ambiguity-averse traders depends on how quickly and completely they obtain the relevant missing information that is the underlying driver of the ambiguity. In the context of my study, after an increase in the VIX, it is possible that there will be no asymmetric responses to bad and good news because the missing information is quickly discovered and traded on. In this case, an increase in the VIX would yield symmetric responses to earnings news following high trading volume. On the other hand, if missing information is not quickly discovered, then an increase in the VIX would yield asymmetric responses and low trading volume from ambiguity-averse traders.

I examine responses to bad and good news earnings news within each VIX partition for firms classified on the basis of abnormal trading volume during an earnings announcement window. To form my classification, I first separate firms on whether the VIX increases or decreases immediately before a given earnings announcement. I then further divide firms within each group into those experiencing either high or low abnormal volume over the respective three-day event window. My prediction is that the observed asymmetry in bad and good news responses will be most evident in the group experiencing both an increase in the VIX and low abnormal volume. The intuition here is that if volume represents information arrival, then information-based trading will resolve ambiguity and, thus, mute the asymmetric response to earnings news.[15]

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[14] Billot, Chateauneuf, Gilboa, and Tallon (2000) also directly examine the relation between ambiguity and trading volume. Billot et al. (2000) use the max-min framework of Gilboa and Schmeidler (1989) to address trading under ambiguity, specifically, with regard to agreement and disagreement among traders. Under the typical Bayesian framework, investors trade only if there is investor disagreement on probabilities. By contrast, Billot et al. (2000) propose a model of ambiguity with shades of grey, where ambiguity-averse investors may not *agree* in the sense of having the same set of possible priors, but may also not *disagree* in the sense they are willing to take bets against each other. Using this model, Billot et al. (2000) show that in a multiple priors (max-min) framework, if ambiguity-averse investors have at least one prior in common, then there is an absence of betting. Finally, Bewley (2002) also proposes a model of trading under ambiguity that uses the concept of "inertia" instead of the max-min model and finds similar results.

To compute trading activity over the event period, I use abnormal turnover (*AbVol*). To determine *AbVol,* I first compute a firm's average turnover (*AvgVol*) over a period of 14 contiguous trading days, ending five trading days prior to the first quarterly announcement in firm *i*'s industry in the current fiscal quarter. This period is chosen to mitigate any potential effects created by other news distorting the firm's normal or average turnover. I then compute the firm's average turnover as follows:

$$AvgVol_{it} = \frac{1}{14}\sum_{s=-14}^{-1} Log\left[\left[\frac{SharesTraded_{it}}{SharesOut_{it}}\right] + 0.000000025\right],$$

where *SharesTraded* is the total number of firm *i*'s shares traded on day *s,* and *SharesOut* is the total number of shares outstanding for firm *i* on day *s*. Following prior research, I add a small constant to avoid taking the log of zero. In a similar manner, I compute the cumulative abnormal turnover, or *AbVol,* for the event window, $t-1$ to $t+1$, where t is the quarterly earnings announcement:

$$AbVol_{it} = \sum_{t=-1}^{1}\left[Log\left[\left[\frac{SharesTraded_{it}}{SharesOut_{it}}\right] + 0.000000025\right] - AvgVol_{it}\right].$$

After obtaining *AbVol,* I rank firm-quarter observations to create a dichotomous variable equal to 1 (0) if a firm quarter observation is above (below) the median. The new dichotomous variable is referred to as $AbVol_{High}$ for firm-quarters above the median and $AbVol_{Low}$ for firm-quarters below the median.

Table 9 presents the results obtained from estimating Equation (1) within each *AbVol* partition. Similar to the results for my VIX beta partition, I find an asymmetric response to bad versus good news following an increase in the VIX for firms with lower trading volumes; this result is significant at the 0.05 level. By contrast, a decrease in the VIX yields symmetric responses for all groups.

Investor Sophistication

In another analysis, I test the robustness of my results to the level of investor sophistication. This analysis draws on the work of Easley and O'Hara (2010), who provide a theoretical model that suggests unsophisticated investors are more likely to behave in a manner consistent with ambiguity aversion. Specifically, they propose that unsophisticated investors are likely to include more extreme distributions within their set of distributions due to their lack of understanding concerning the interworking of the market. Thus, a greater number of unsophisticated investors in an investor set may skew the results toward more extreme asymmetric reactions.

I test this conjecture by using 13-F filings to rank firms on their percent of institutional holdings. I then reestimate the analysis for high and low institutional ownership (*IntOwn*). Consistent with the above conjecture, the results in Table 10 show that the documented asymmetric response to earnings news by the market immediately following an increase in the VIX is more pronounced for firms with a low percent of institutional holdings. Prior research suggests the results between high and low

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[15] While an increase in the VIX potentially reflects ambiguity, it is not possible to distinguish whether the lack of an asymmetric response is attributable to resolution of ambiguity or to the absence of ambiguity. Therefore, I interpret my high-volume results with caution.

TABLE 9

The Presence of Volume and Investor Asymmetric Responses to Earnings Surprises Following Changes in Uncertainty

	Dependent Variable: *Ret*			
	ΔVIX^{-}		ΔVIX^{+}	
Variables	$AbVol_{Low}$	$AbVol_{High}$	$AbVol_{Low}$	$AbVol_{High}$
Badnews	0.4663***	0.8883***	0.7944***	1.2440***
	(0.1088)	(0.1554)	(0.1131)	(0.1738)
Goodnews	0.3972***	0.7865***	0.2372***	0.8340***
	(0.0549)	(0.0703)	(0.0611)	(0.0777)
VIX	0.0001	−0.0003**	−0.0002**	−0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Size	0.0002	−0.0025***	0.0007*	−0.0016***
	(0.0003)	(0.0005)	(0.0004)	(0.0005)
Ret_{pre}	−0.0137*	−0.0107***	−0.0019**	−0.0241***
	(0.0071)	(0.0035)	(0.0009)	(0.0042)
#Analysts	0.0001	0.0002**	0.0002**	0.0003**
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
n	13,764	13,424	12,162	11,628
Test of Asymmetry:				
Badnews = *Goodnews*	0.0691	0.1018	0.5872***	0.4100*

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*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table 9 presents the results from OLS regressions of returns on quarterly earnings news and control variables from 1986–2007 estimated by ΔVIX and *AbVol*. Regressions also include year fixed effects, and the significance level is calculated using the error clustered along firm and time.

Variable Definitions:

ΔVIX^{+} (ΔVIX^{-}) = an increase (decrease) in the VIX two days prior to the event window;

AbVol = an indicator variable set to 1 if the abnormal turnover for the event window is above the median, and 0 otherwise;

Ret = three-day (t−1 to t+1) cumulative market-adjusted return for firm i over the quarterly earnings announcement date t;

Badnews (*Goodnews*) = *UE* when $UE < (>) 0$, and 0 otherwise;

VIX = average level of the VIX over the week prior to the window in which the change in the VIX is measured;

Size = natural logarithm of the market value of equity;

Ret_{pre} = firm's return over the quarter leading up to the earnings announcement event window; and

#Analysts = number of analysts following the firm over the quarter.

IntOwn may also be consistent with short-selling constraints or limits to arbitrage (D'Avolio 2002). However, my finding of a symmetric response to good and bad news for low *IntOwn* firms following a decrease in the VIX is inconsistent with a limit to arbitrage hypothesis.

Additional Analyses

In a final exploratory analysis, I draw on prior work by Jiang et al. (2005) and Zhang (2006), who investigate the effects of information uncertainty (*IU*) on expected returns. In both studies, information uncertainty, or *IU*, is defined as the amount of variance in firm valuation that makes it difficult for investors to value a firm. Zhang (2006) explains that this valuation uncertainty, or risk, can arise because of either fundamental volatility or issues related to information quality. While the

TABLE 10

Institutional Ownership and Investor Asymmetric Responses to Earnings Surprises Following Changes in Uncertainty

	Dependent Variable: *Ret*			
	ΔVIX^-		ΔVIX^+	
Variables	**Low *IntOwn***	**High *IntOwn***	**Low *IntOwn***	**High *IntOwn***
Badnews	0.8321***	0.4813***	1.1079***	0.8894***
	(0.1176)	(0.1516)	(0.0136)	(0.1554)
Goodnews	0.7667***	0.6116***	0.5998***	0.6000***
	(0.0674)	(0.0669)	(0.0706)	(0.0756)
VIX	−0.0001	−0.0002**	−0.0002**	−0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Size	−0.0008**	−0.0016***	0.0005	−0.0012**
	(0.0004)	(0.0004)	(0.0004)	(0.0005)
Ret_{pre}	−0.0064***	−0.0271***	−0.0020**	−0.0298***
	(0.0022)	(0.0039)	(0.0009)	(0.0044)
#Analysts	0.0025**	0.0001	0.0002	0.0003***
	(0.0001)	(0.0001)	(0.0002)	(0.0001)
n	13,445	13,741	12,038	11,742
Test of Asymmetry				
Badnews = *Goodnews*	0.0654	0.1303	0.5081***	0.2894*

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table 10 presents the results from OLS regressions of returns on quarterly earnings news and control variables from 1986–2007 estimated by ΔVIX and *IntOwn*. Regressions also include year-fixed effects and the significance level is calculated using the error clustered along firm and time.

Variable Definitions:
ΔVIX^+ (ΔVIX^-) = an increase (decrease) in the VIX two days prior to the event window;
IntOwn = an indicator variable set to 1 if the institutional ownership of the firm is above the median, and 0 otherwise;
Ret = three-day ($t-1$ to $t+1$) cumulative market-adjusted return for firm *i* over the quarterly earnings announcement date *t;*
Badnews (*Goodnews*) = *UE* when *UE* < (>) 0, and 0 otherwise;
VIX = average level of the VIX over the week prior to the window in which the change in the VIX is measured;
Size = natural logarithm of the market value of equity;
Ret_{pre} = firm's return over the quarter leading up to the earnings announcement event window; and
#Analysts = number of analysts following the firm over the quarter.

prior literature does not directly examine the relation between *IU* and ambiguity, as an exploratory analysis, I investigate the cross-sectional effects between ambiguity and information uncertainty.

Following Jiang et al. (2005) and Zhang (2006), I use two proxies for information uncertainty: volatility of returns and volatility of cash flows. First, I compute the volatility of returns (*RetVOL*) for a firm as the standard deviation of the firm's market-adjusted return over the quarter ending ten days prior to the earnings announcement window. Second, to compute the volatility of cash flows (*cfoVOL*), I calculate the standard deviation of operating cash flows for the firm over the prior 20 quarters. I then rank both the return and cash flow volatility measures at the median, where *High* (*Low*) *IU* firms are defined as above (below) the median.

In Table 11, Panels A and B present the results of the analysis. The results show that for both *RetVOL* and *cfoVOL*, the asymmetric effects are concentrated in the set of high *IU* firms. These results are somewhat intuitive, as firms that experience high information uncertainty are likely to be

TABLE 11

Firm-Level Information Uncertainty (*IU*) and Investor Asymmetric Responses to Earnings Surprises Following Changes in Macro-Uncertainty

Panel A: *IU* = *RetVOL*

	Dependent Variable: *Ret*			
	ΔVIX^-		ΔVIX^+	
Variables	**Low *RetVOL***	**High *RetVOL***	**Low *RetVOL***	**High *RetVOL***
Badnews	0.7549***	0.6460***	0.7927***	1.2342***
	(0.1133)	(0.1449)	(0.1213)	(0.1559)
Goodnews	0.7387***	0.6568***	0.7303***	0.5328***
	(0.0629)	(0.0653)	(0.0642)	(0.0758)
Controls	Included	Included	Included	Included
n	13,414	13,774	12,145	11,645
Test of Asymmetry				
Badnews = *Goodnews*	0.0162	0.0108	0.0624	0.7014***

Panel B: *IU* = *cfoVOL*

	Dependent Variable: *Ret*			
	ΔVIX^-		ΔVIX^+	
Variables	**Low *cfoVOL***	**High *cfoVOL***	**Low *cfoVOL***	**High *cfoVOL***
Badnews	0.8062***	0.5200***	0.7832***	1.3059***
	(0.1454)	(0.1510)	(0.1592)	(0.1654)
Goodnews	0.6322***	0.7991***	0.6923***	0.5417***
	(0.0796)	(0.0721)	(0.0822)	(0.0798)
Controls	Included	Included	Included	Included
n	11,798	11,792	10,354	10,359
Test of Asymmetry				
Badnews = *Goodnews*	0.1740	0.2791	0.0909	0.7642***

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*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table 11 presents the results from OLS regressions of returns on quarterly earnings news and control variables from 1986–2007 estimated by ΔVIX and *IU*. Proxies for *IU* are the return (*RetVOL*) and cash flow (*cfoVOL*) volatility of the firm. Regressions also include year fixed effects, and the significance level is calculated using the error clustered along firm and time.

Variable Definitions:

ΔVIX^+ (ΔVIX^-) = an increase (decrease) in the VIX two days prior to the event window;

RetVOL = an indicator variable set to 1 if the standard deviation of the firm's market-adjusted return over the prior quarter is above the median, and 0 otherwise;

cfoVOL = an indicator variable set to 1 if the standard deviation of the firm's operating cash flow over the prior 20 quarters is above the median, and 0 otherwise;

Ret = three-day (t–1 to t+1) cumulative market-adjusted return for firm *i* over the quarterly earnings announcement date *t;*

Badnews (*Goodnews*) = *UE* when $UE < (>) 0$, and 0 otherwise;

VIX = average level of the VIX over the week prior to the window in which the change in the VIX is measured;

Size = natural logarithm of the market value of equity;

Ret_{pre} = firm's return over the quarter leading up to the earnings announcement event window; and

#Analysts = number of analysts following the firm over the quarter.

the firms where there is the most scope for ambiguity to affect the decision-making process of investors.

V. CONCLUSION

This paper investigates how changes in the macro-economic environment impact the way in which investors process earnings information. In particular, I examine the role of *ambiguity* in shaping the responses of stock market participants to firm-specific information releases. Using changes in the VIX as an empirical proxy for ambiguity, I find that an increase in the VIX leads investors to place more weight on bad earnings news than on good earnings news. By contrast, I find that a decrease in the VIX leads investors to place equal weight on both bad and good earnings news. These results are consistent with the predictions of ambiguity theory (Epstein and Schneider 2008; Hansen and Sargent 2010), as well as previous experimental results (Ellsberg 1961). In a set of additional analyses, I find that my results are robust to controlling for leverage effects, volatility feedback effects, market betas, investor sentiment levels, and investor perceptions of the state of the economy. I also find that the effects vary depending on firm-specific susceptibility to the effects of uncertainty shocks.

Finally, I find a relation between the observed asymmetric effects and the level of trading volume. Using both firm-specific and market-wide abnormal trading volume as *ex post* proxies for information arrival, I find that firms with low (high) levels of volume after an increase in the VIX exhibit an exacerbated (mitigated) asymmetric response. This result suggests an interesting link across ambiguity, information, and trading volume.

Overall, this paper contributes to the literature by providing large-scale empirical evidence that changes in ambiguity affect how investors process information. In doing so, my results provide insight into the fundamental workings of complex capital markets. The results also suggest a fundamental economic reason for observed asymmetric responses to accounting information.

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Book Reviews

Stephen A. Zeff, Editor
Rice University

Stephen A. Zeff, Rice University, continues as the book review editor for the March and May issues of the journal.

Two copies of books for review should be sent to the incoming Book Review Editor: Professor Gary C. Biddle, Room 1305, K. K. Leung Building, The University of Hong Kong, Pokfulam, Hong Kong. The policy of *The Accounting Review* is to publish only those reviews solicited by the Book Review Editor. Unsolicited reviews will not be accepted.

ICAEW FINANCIAL REPORTING FACULTY, *Financial Reporting Disclosures: Market and Regulatory Failures* (London, U.K.: ICAEW Financial Reporting Faculty, 2013, ISBN 978-0-85760-296-1, pp. ix, 79).

The stated objective of the Institute of Chartered Accountants in England and Wales (ICAEW) in this monograph is to examine "sources of unhappiness" about "widespread complaints of information overload in financial reporting" and "identify a number of ways in which the regulatory framework should be changed to address current concerns" (p. vi). I begin with a brief summary of the book. The monograph consists of six chapters along with four appendices that summarize more technical material such as details of research studies cited and disclosure frameworks proposed by the Financial Accounting Standards Board (FASB), the Securities and Exchange Commission (SEC), and other regulatory bodies. The six chapters are organized as follows. Chapter 1 introduces the concept of disclosure overload as "too much information," because it is common for companies to produce annual reports far in excess of 100 pages. It goes on to frame this debate in the context of (1) the information explosion that we have witnessed in general with the advent of the Internet; (2) a rising expectation in society about transparent reporting in many areas (e.g., information about pharmaceuticals to consumers); and (3) the incentives of the stakeholders associated with corporate financial reporting (e.g., auditors, standard setters, lawyers, competitors, managers, and politicians).

Chapter 2 suggests that firms have incentives to provide financial disclosures to outsiders and that disclosure overload might have occurred in any case even in the absence of mandatory disclosure regulation, given the advances in technology and expectations about transparency. Chapter 3 summarizes the classic arguments for market failures associated with the production of financial information and how regulation addresses these market failures. For instance, it is often argued that disclosure is a public good that is consumed by users who do not necessarily pay for it. Hence, voluntary disclosure will be under-produced, in the absence of regulation. Chapter 4 recognizes that regulation potentially creates incentives for firms to game the rules and for regulators to serve their own ends. Chapter 5 discusses regulatory failures, the more important of which impose uniform reporting requirements on diverse firms to meet the needs of inherently diverse users. Such regulatory failures, in turn, potentially lead to disclosure overload. Chapter 6 discusses what should be done to address these problems and potentially reduce disclosure overload.

Overall, I enjoyed reading the monograph and found it to be even-handed and a great reference for questions that are often raised about why financial disclosure is mandated. I organize the review of the monograph around four issues: (1) the importance of the question examined; (2) does mandatory disclosure improve outcomes; (3) the novelty of the material presented; and (4) a critique of the recommendations proposed by the monograph.

IMPORTANCE OF THE QUESTION EXAMINED

It is not obvious to me that the complaint of "disclosure overload" is a legitimate and a credible concern among the stakeholders associated with financial reporting. Although the monograph is often at pains to point out that it is meant for an international audience, I cannot help but think about the U.S. context because of (1) my background, and (2) some of the best data to evaluate claims of disclosure overload are available in the U.S.

At least in the U.S., potentially excessive disclosure stems from the existing institutional arrangements, which are difficult to alter in the short run and perhaps even in the long run. Much of the disclosure is boilerplate in nature and is excessively detailed to ward off the threat of a class action lawsuit. Assuming that the existing legal system is difficult to change, self-interested firms are perhaps better off providing excessive disclosure even if such actions reduce the probability of a lawsuit only to a small degree. Sophisticated users always have the option to ignore disclosures they might perceive as unnecessary or unreliable. For instance, there is evidence that creditors have begun under-emphasizing the role of the balance sheet, which has become increasingly fair-value oriented on account of FASB pronouncements, in writing debt covenants (Demerjian 2011). Unsophisticated investors are perhaps better off holding mutual funds that mimic the market or particular sectors of the economy and, hence, do not have to worry as much about excessive financial reporting disclosures. Standard setters have little incentive to write a standard saying that their job is done. As a chief financial officer once remarked to me, "[T]he FASB could have stopped after writing the conceptual framework" (Dichev, Graham, Harvey, and Rajgopal 2013). Auditors benefit from complex regulation as the market for their advisory services expands (e.g., fair-valuing intangible assets in a business combination or evaluating whether an asset is impaired relative to the fair value of the asset). In sum, there is not much incentive for existing stakeholders to address the issue of "disclosure overload," even if it were a first-order problem.

DOES MANDATORY DISCLOSURE IMPROVE OUTCOMES?

The question of disclosure overload is relevant only if we believe that mandatory disclosure adds value. The monograph claims that "there is abundant research evidence of the benefits of disclosure (which we discuss in Appendix 3)" (p. 3). I am not as convinced as the ICAEW of the benefits of mandatory disclosure.

Before proceeding, it seems useful to ask whether scholars have been able to identify a serious, systemic information-related problem that mandatory regulation can correct. I could not find such evidence in the monograph. This is not to argue that we cannot find instances where firms have harmed stakeholders by exploiting private financial information. However, it is not obvious that mandatory standard setting has successfully mitigated this problem.

One of the key benefits cited is the purported link between mandatory disclosure and lower cost of capital. Several of these cited studies do not seem to discuss why firms would leave money on the table and not disclose financial information voluntarily, if such behavior materially reduces cost of capital. In my interactions with practitioners, I have found that they often talk about a potential link between disclosure and share prices, not about cost of capital. As is well known, decoupling the impact of future cash flows from cost of capital effects in share price is a difficult, if not an impossible, task. I wonder whether the early work on the potential link between disclosure and cost of capital has become a convenient excuse for regulators and standard setters to justify their mandate to continue writing disclosure regulations.

It is not obvious that mandatory disclosure improves outcomes for investors in other settings that have mandated more financial disclosure such as the SEC's 1933 Truth-in-Securities Act (Stigler 1964; Benston 1973; Jarrell 1981). Greenstone, Oyer, and Vissing-Jorgensen (2006) found that shareholders valued the 1964 Securities Act Amendments that extended mandatory disclosure regulations to large firms traded over the counter, but the authors cautioned that they could not comment on welfare gains, as shareholder gains might be offset by managers' losses. Ferrell (2003), however, reported that a simple comparison of stock returns in the years before and after the 1964 Act was implemented showed no increase in stock returns. Gomes, Gorton, and Madureira (2007) found that Regulation Fair Disclosure had the unintended consequence of causing a welfare loss to small firms because they faced a higher cost of capital. Coates and Srinivasan (2014) find similar mixed evidence in their survey of the literature on the impact of the Sarbanes-Oxley Act of 2002. Somewhat surprisingly, none of these studies is cited in the monograph.

Finally, it is not obvious that the mandatory disclosure of information has improved social outcomes in other settings such as patient safety, plant closing disclosure, nutritional labeling, toxic releases disclosure, and workplace hazards disclosure (Fung, Graham, and Weil 2008), and even in areas that are claimed as a policy success in mandatory disclosure such as mortgage lending disclosure and restaurant hygiene disclosure (Winston 2006).

NOVELTY OF THE MATERIAL PRESENTED

Much of the material reviewed in the first three chapters of the monograph can be found in existing academic literature surveys, research articles, and books. To summarize, Chapter 1 claims that no market or regulatory forces can fully address the impact of three factors on reporting: (1) subjective judgments of preparers, (2) managers' "self-reporting bias," and (3) firms' proprietary costs. Chapter 2 argues that disclosure overload could occur even in the absence of regulation due to changes in the nature of markets, product, and capital, and in technology. Chapter 3 suggests three types of "market failure" that might motivate a role for regulation in reporting: (1) reporting is a public good and will, hence, lead to under-production of such information, (2) mandatory standardization can enhance comparability of disclosures, and (3) regulatory regimes are likely to make disclosures more reliable and credible.

The role of agency costs, proprietary costs, and the use of managerial discretion in financial reporting, discussed in Chapter 1 here, has been extensively researched (see Fields, Lys, and Vincent [2001] and Beyer, Cohen, Lys, and Walther [2010] for literature reviews). Watts and Zimmerman (1986) discuss the characterization of market failure in the market for disclosure and the potential role for standardization (the material covered in Chapter 3 in the monograph). Similar to Chapter 5 in the monograph, preparers' distaste for the reporting requirements imposed by the FASB has been covered extensively in Dichev et al. (2013).

CRITIQUE OF THE RECOMMENDATIONS

The recommendations made in the monograph in Chapter 6 raise some obvious and thorny questions. The overarching concern is whether a regulatory body such as the ICAEW can even be objective about questions related to whether regulation in disclosure is effective. Most of the recommendations made in the monograph assume that the regulator can somehow efficiently adjudicate between competing interest groups in a socially optimal manner.

One of the specific recommendations of the ICAEW is that standard setters should assign more weight to the views of equity holders, the owners, who meet the cost of disclosure requirements. How exactly should they do that? Who exactly are these mythical owners? Most of the larger shareholders, such as institutional investors, already lobby the FASB and influence disclosure requirements. Do we have evidence to suggest that the FASB under-weights the input of such shareholders? These recommendations are particularly puzzling, because the ICAEW recognizes the difficulty in identifying the so-called "mythical user" (Young 2006) on page 5, but it continues to succumb to the same fallacy when it makes its own recommendations.

Another recommendation by the ICAEW suggests that standard setters should establish a framework to ensure that disclosures are required only when they are needed and that standard setters should regularly review the rules to weed out excessive disclosure. I doubt whether any standard setter would admit to requiring firms to make excessive disclosure. In their minds, all the disclosure requirements they approve add value. And, can we trust standard setters to correct their past mistakes, given that past standards have potentially caused the disclosure overload problem in the first place?

A recommendation, along similar lines, suggests that enforcement agencies and auditors should clarify that they will not take action against firms that omit immaterial disclosures. Do we have evidence to suggest that the SEC overzealously penalizes firms for omitting immaterial disclosures? The bigger concern is that their enforcement of even material disclosure and reporting issues is hobbled by resource constraints (Kedia and Rajgopal 2011) or potential regulatory capture (Correia 2014; DeHann, Kedia, Koh, and Rajgopal 2014).

Finally, the ICAEW recommends that preparers and users should engage directly to discuss voluntary disclosure rather than rely on standard setters to introduce new disclosure requirements. This recommendation again begs the question of (1) who is the "user," and (2) why do we need a standard-setting body in the first place if users and preparers could indeed coordinate disclosure concerns on their own?

An often overlooked issue with these recommendations is that regulators or standard setters have to adjudicate on wealth transfers across several interest groups and, usually, the more powerful interest group wins. This political reality on these recommendations makes me wonder whether the ICAEW's recommendations have any real bite.

In conclusion, the monograph is an interesting read, but I walked away unconvinced that disclosure overload is a first-order concern that we can do anything about.

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SHIVARAM RAJGOPAL
Professor of Accounting
Emory University

JACOB SOLL, *The Reckoning: Financial Accountability and the Rise and Fall of Nations* (New York, NY: Basic Books, 2014, ISBN: 978-0-465-03152-8, pp. xvii, 276).

Werner Sombart (1924) was the first to advance the hypothesis that double-entry bookkeeping (DEB) was a necessary institution for the development of modern capitalism. Basil Yamey (1964, 117) summarizes Sombart's hypothesis:

> [B]y transforming assets into abstract values and by expressing quantitatively the results of business activities, double-entry bookkeeping clarified the aims of acquisitive business; moreover, it provided the rational basis on which the capitalist could choose the directions in which to employ his capital to best advantage; and finally, it made possible the separation of the business firm from its owners and hence the growth of large joint-stock businesses.

Max Weber and Joseph Schumpeter have echoed Sombart's view of DEB (Most 1972; Winjum 1972). Other work suggests that the recordkeeping function of accounting may be a positive force at even earlier stages in an economy's development (Basu and Waymire 2006). Ultimately, understanding the role of basic accounting institutions in economic development will likely require contributions from multiple disciplines, including law and economics, sociology, psychology and neuroscience, and history. Jacob Soll's *The Reckoning* provides a historical analysis that contributes significantly to this undertaking.

The main focus of this book, as the author states in the introduction, is to explore a moral tension that has surfaced throughout the history of modern accounting:

> People have known how to do good accounting for nearly a millennium, but many financial institutions and regimes have just chosen not to do it. Those societies that have succeeded are not only those rich in accounting and commercial culture but also are the ones that have worked to build a sound moral and cultural framework to manage the fact that humans have a regular habit of ignoring, falsifying, and failing in accounting. This book examines why a lesson so simple has so rarely been learned. (p. 12)

Soll tackles this important issue from the perspective of a historian doing qualitative analysis. This approach allows the historian to cast a wide net in determining what constitutes evidence and how it is to be interpreted, and offer a complementary approach to more quantitative empirical work. Soll's book helps us to better see the "big picture" of how the historical evolution of DEB shaped, and has been shaped by, cultural changes reflected in the economic, social, and religious institutions of human societies since the 14th century.

After a brief introduction, Chapter 1 sets the stage by reviewing the political economy of accounting before 1400. This chapter situates DEB as an innovation with the potential to improve on accounting functions that had existed for thousands of years. Chapters 2 and 3 provide contrasting views of the morality play at work in DEB through two case studies. Chapter 2 ("For God and Profit: The Books According to Saint Matthew") tells the story of Francesco Datini, who rigorously applied DEB to a vast and growing business centered on banking and international trade. Datini was the "good accountant" whose books were "overwhelming" in scope and number. Most importantly, the historical context for Datini's business lies in the religious tradition of Catholicism, which viewed the pursuit of business profit as sinful. Chapter 3 ("Medici Magnificence: A Cautionary Tale") tells the tale of Cosimo de' Medici, a Florentine banker who amassed great wealth, but whose heirs struggled to manage the bank's far-flung finances, in part because of political connections that today might be described as "crony capitalism." In combination, Chapters 2 and 3 are central to the book's thesis that DEB is a moral enterprise that requires both hard work and extraordinary self-discipline to implement successfully.

Luca Pacioli enters the story in Chapter 4 ("The Mathematician, the Courtier, and the Emperor of the World"). This chapter tells us something about Pacioli's background and the circumstances under which he wrote the *Summa*. Soll describes Pacioli as the "father of accounting" whose manual was "its founding work" (p. 49). More importantly, Soll establishes the idea that DEB as depicted by Pacioli was a moral undertaking where the "balance of account books represented the moral equilibrium of God" (pp. 50–51). He also recounts how Pacioli's *Summa* was not a commercial success initially (p. 55) and reviews the difficulties faced by early statesmen, politicians, and businessmen who implemented DEB with varying degrees of success (pp. 55–69).

Chapters 5 to 10 provide country-specific reviews of accounting and economic development during different time periods: Holland (Chapter 5), France under Louis XIV (Chapter 6), England (Chapters 7 and 8), 18th century France before and during the French Revolution (Chapter 9), and the United States around the time of the American Revolution (Chapter 10). All told, these chapters further the narrative by providing more detailed analyses showing how the same moral dilemma associated with DEB surfaced repeatedly between 1500 and 1900 in several countries.

Chapter 11 provides a discussion of accounting's role in the emergence of 19th century British and American railroads. This chapter does an excellent job of describing how large railroad organizations transformed societies, influenced political institutions, and fostered the emergence of accounting as a modern profession. Chapter 11 also serves in part as a segue into a captivating discussion of how accounting is represented in the literature of Charles Dickens and other authors (Chapter 12, "The Dickens Dilemma").

Soll (p. 179) cites Dickens to illustrate the moral tension at the heart of DEB:

> Of all the authors of the nineteenth century, Charles Dickens had the most vivid view of accountants and accountability. In Dickens's world, the accountant had been reduced to a good-hearted but hapless clerk, a malicious swindler, or a nightmarish bureaucrat. Accountants could be good men like Bob Cratchit, the father of Tiny Tim in *A Christmas Carol* (1843), who loyally kept the books of the counting house or bank of the miser Ebenezer Scrooge. Both Scrooge and his ghostly partner in banking, Jacob Marley, had trained as accountants. . . . Marley's ghost appeared to Scrooge, bound by a chain: "It was long, and wound about him like a tail; and it was made (for Scrooge observed it closely) of cash-boxes, keys, padlocks, ledgers, deeds, and heavy purses wrought in steel." Not just money, but the ledgers and deeds of accounting had ensnared the wily banker and imprisoned his soul. Scrooge risked the same were he not to make amends, in the logic of Pacioli, paying to the poor on Christmas to balance his accounts with a moral, Christian God.

Chapter 13 and the final chapter titled "Conclusion" serve an integrative purpose. These chapters are very good, but they do seem somewhat incomplete. Chapter 13, appropriately titled "Judgment Day," reviews the American experience in the 20th century. It chronicles various private sector scandals of this era but does not link back to accounting discipline in the public sector, as was the case in several earlier chapters. For example, some, like Warren Rudman and David Walker, have tried to warn of the potentially disastrous consequences of profligate spending by American politicians over the last several decades. The final chapter is brief and runs only three pages. That is both positive and negative. It is positive in that Soll does not make sweeping generalizations based on his historical survey. It is possibly unfortunate in that a few more speculations about implications for the future would have been welcomed. Nonetheless, the concerns about the final two chapters are minor in relation to the value delivered by this book.

Overall, Soll successfully sets up his history of double-entry bookkeeping as a morality play. This is, in my opinion, the book's primary strength. The idea that the birth of DEB is linked inextricably with the foundations of business ethics and morality is compelling. The moral conflict faced by those seeking accountability is apparent in the history that Soll pulls together. Political and business leaders may not provide proper accounting because it is difficult; it requires sustained effort and diligence as illustrated by Datini over 600 years ago. Soll brings the moral conflict inherent in DEB to life in his descriptions of "buck-passing" by leaders who say they value accountability but lack the discipline to actually deliver it.

In terms of the book's contribution, it is perhaps helpful to contrast it with other books on the subject. It is similar to James Aho's (2005) *Confession and Bookkeeping* in that it sets up the history of DEB as a moral dilemma. It also explores issues similar to Winjum's (1972) study of the relation of DEB and economic development. However, Soll's historical analysis is broader and extends beyond the specific times and places considered by Aho (2005) and Winjum (1972): in the case of Aho (2005), the Catholic Church in Italy during the Renaissance, and in the case of Winjum (1972), Great Britain from 1500 to 1750. A more direct competitor is Jane Gleeson-White's (2012) *Double Entry: How the Merchants of Venice Created Modern Finance*. Soll's book is vastly superior to Gleeson-White's (2012) in terms of historical breadth and depth, as well as linking the analysis more accurately with broader social changes.[1]

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[1] Gleeson-White's (2012) book runs off the rails in the final chapters when she tries to link DEB with so-called market failures linked to third-party costs (i.e., negative externalities).

Overall, I recommend Jacob Soll's *The Reckoning: Financial Accountability and the Rise and Fall of Nations* enthusiastically and without reservation. But to what audience? First, this book will be useful in helping serious undergraduate accounting students understand the importance of their future work and its historical origins. (For the same reasons, it might be of interest to practicing accountants.) Indeed, I think this book would be a perfect companion to Brown and Palmrose's (2005) *Thog's Guide to Quantum Economics: 50,000 Years of Accounting Basics for the Future* in an undergraduate seminar on accounting theory. Second, the book should be read by accounting doctoral students and seasoned faculty members. While the book may not have any direct bearing on any specific analytical or empirical research project, it does provide the serious scholar with a perspective that I think is helpful. After all, man does not live by standard errors alone! Finally, if any members of the general public, whose interactions with DEB are limited, wander by accident onto the pages of *The Accounting Review,* I recommend Jacob Soll's book to them as well. It is well-written and fun to read. If nothing else, Soll's book reminds us of a truth that is easy to lose sight of: those human institutions that are genuinely important for our survival are sometimes not recognized as such. The ubiquity of accounting and recordkeeping is a bit like breathing in that we notice it only when it stops working effectively.

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GREGORY B. WAYMIRE
Professor of Accounting
Emory University

IZUMI WATANABE (editor), *Fair Value Accounting in Historical Perspective* (Tokyo, Japan: Moriyama Shoten, 2014, ISBN 978-4-8394-2139-7, pp. x, 270).

If you are not an Anglo-American country, which set of accounting standards do you adopt? Do you go with the International Accounting Standards Board (IASB) or Financial Accounting Standards Board (FASB), or do you stay with, and enhance, your own national accounting standards? What are the consequences for accounting practice, and what legacy are you acquiring when you harmonize with International Financial Reporting Standards (IFRSs)? These are essentially the questions being asked about fair value accounting by Japanese academics in this book, evaluating the extent to which fair value is relevant and welcome when making the choice of which set of standards to adopt.

The most important insight comes from the penultimate chapter in the book. Ono discusses debates surrounding the treatment of affiliate stock issues acquired as business investments under an illiquid market as an example of a divergence from fair value accounting. The insight, however, applies to all and any of the versions of fair value accounting that we have in our current sets of standards, and to the continuing debates around them. It is that what we have now is "perhaps best understood as the vestiges of the struggles of our ancestors" (p. 240).

Ten out of 11 chapters of the book give a history of the ancestral struggles in Britain and the United States over several centuries to deal with assets, liabilities, gains, and losses that are not easily represented in financial reports by historical cost. For the non-Japanese reader, the book provides at least one very valuable service. It condenses a rich heritage of recent Japanese academic thought and historical research on the subject of fair value accounting, published originally in Japanese, and presents it systematically for English-speaking colleagues and students. There is extensive use of publications in the English language as well, and the majority of the chapters draw on examples from Western countries. Chapters 1, 9, and 10 do give examples of the use of fair values in Japan, and one of the minor disappointments of the book, for those who may be very familiar with the British and U.S. cases used, is that there is not more information about the struggles in Japan itself to find a means of expressing the current or future value of historical transactions.

The book is a set of disparate essays held together as a project that sets out to understand where fair value came from. Japan's Financial Services Agency announced in 2010 that it would adopt IFRS 9, *Financial Instruments,* for use on a voluntary basis for some listed Japanese companies (Sanderson 2010). At a time when the IASB and FASB were failing to come to an agreement on fair value and convergence in financial reporting, Japan plumped for the IASB, having expressed frustration at the lack of progress on the issue. In 2010, Japan was ahead of Europe in making a decision to adopt IFRS 9 (Christodoulou 2010), which has been completed and agreed upon only in 2014, and is effective for annual periods beginning on or after January 1, 2018.

Section 1 (Chapters 1–4) offers literature reviews of the current state of fair value accounting and the technical issues that it presents for preparers and users of financial statements. Overall, the four chapters are critical of fair value accounting. Toshifumi Matsumoto in Chapter 1 discusses concerns with the recognition of distributable income in the "patchwork pattern" of mixed methods accounting: the problem, which he rightly isolates, is how you provide information for decision-making generally while preserving some prudence in the distribution of dividends. Norio Takasu (Chapter 4) also explores this idea, seeing the potential of dividends from unrealized gains as the main point of conflict in the use of fair value accounting.

Section 2 (Chapters 5–9), covers historical cases of the use of market values in accounts in the U.K. and the U.S. from the seventeenth century. Takeshi Sugita (Chapter 5) examines the British East India Company and the use of market values in determining earnings and dividends. Chapter 7 by Kei Okajima is an account of asset verification and auditing practices in the late nineteenth and early twentieth centuries. In this period, the difficulty was how to value assets and the gradual acceptance that the physical condition of the asset was needed as well as the historical transaction cost. However, Okajima does not fully develop the link to fair value accounting now but rather carefully sets out the arguments leading up to the acceptance of physical verification of assets. Izumi Watanabe (Chapter 6) examines the use of revaluations in British accounts from the seventeenth century and the high level of verifiability provided by the notion of the *Bilanzio* (an early form of the balance sheet) from double-entry bookkeeping. This provides the basis for his more polemical writings in the foreword and final chapter.

For Watanabe, the editor of this book, the debates in Japan, the U.S., and countries adopting IFRS signaled a need to reexamine the basis of fair value measurement and the original role of double-entry bookkeeping. He is overtly skeptical of the move towards decision-usefulness which is encapsulated in fair value accounting, and states forcefully, both in the foreword and the polemical last chapter, that decision-usefulness departs from the "true nature" of accounting. His stated desire is "to sound a warning bell with the hammer of history to show the perilous journey modern accounting is taking with its core shifts from reliability to usefulness, and from verifiability to relevance" (pp. v–vi) in the face of Japan's acceptance of fair value accounting. The lesson from history offered here appears to be that accounting for assets and liabilities has always lured accountants away from the true essence of accounting and into the dangerous reefs of mixed model reporting.

How seriously should we take this argument? The rhetoric of the beginning and end of the book makes lively and provocative reading, but it sits rather oddly with the careful, scholarly detail of all of the other chapters in this edited collection. However, the book has similar themes to other papers in English-language journals published since 2011, on the desirability of decision-usefulness, whether an essence of accounting exists, and the legitimacy of mixed bases for financial reporting. Several authors, like Watanabe, are provoked by dismay and even anger over the realization that mark-to-market or mark-to-model accounting could be implicated in recent financial crises and scandals.

The authors look back to the origins of accounting in double-entry bookkeeping (DEB) to find the essence of accounting, and then forward to examine the varying ways in which accountants have tried to deal with the problem of accounting for assets and liabilities. They consider DEB and the *Bilanzio* to be based on principles of reliability and verifiability which are lost in the mixed methods approach that fair value, as we have now under the IASB, has brought about, and appear to be advocating a return to historical cost, stewardship accounting. However, the book as a whole belies the final plea for moving back from fair values by demonstrating the persistent need for information about assets and liabilities not given by historical cost accounting.

The immediate problem is the assumption that accounting has an essence, and that this essence can be found in DEB. The use of accounting precedes DEB, of course, and Ezzamel (2012) shows that the role of accounting in society from ancient times (he examines Ancient Egyptian accounting) is to bring order. Simply, decision-usefulness, flawed though it may be (Williams and Ravenscroft 2014) has as much claim to be the essence of accounting as the basics of accounting in Western medieval times. Decision-usefulness is the outcome of discussions on conceptual frameworks, and, at least at a lay level, surveys report that users find financial reports useful (Rutherford 2013) and that normative accounting research aimed at improving financial reporting has a basis in pragmaticism. The desire to pin down "essence" is almost fallacious, or, at best, a futile exercise. Hopwood (2007, 1,367) states plainly that "Accounting has been a craft that has had no essence. It has changed significantly across time, adopting new forms, methods, and roles." Miller and Power (2013) expand on this, observing that we need to understand that accounting has several roles in society—territorializing, mediating, adjudicating, and subjectivizing in order to organize and economize social life.

In other words, accounting practice reflects society and organizational reality as it is, and is not immutable. This is indeed what *Fair Value Accounting in Historical Perspective* shows: the different problems of the times that compel the preparers of financial statements to try to communicate to their shareholders and lenders the precise nature of their business transactions. The main weakness of the book is that nearly all of the examples are on manufacturing, accounting scandals such as Enron, Lehman Brothers, and sub-prime mortgages, and the technicalities of measuring fair value. The project on which the book is based side-steps any meaningful discussion about financial instruments, despite the debate in Japan being centered on the adoption of IFRS 9, *Financial Instruments*. To understand why the world "became strikingly devoted to fair value accounting" (p. 264) surely requires understanding why users need information about the assets and liabilities that are underwritten by financial instruments, and why, as Young (1996) points out, the use of financial instruments deliberately blurs the DEB demarcations of asset and expense, liability and capital.

There is a very brief exposition by Hideki Kubota in Chapter 9 on the acceptance of valuation at current price and fair value measurement in Japan after World War II. He sees this as the period in which business accounting was modernized in Japan, and when two forms of accounting coexisted under the Commercial Code (or Companies Act) and Securities and Exchange Law. A project to extend company law accounting requirements by creating accounting standards that would allow for accounting of financial instruments began in the 1930s. The difference, it appears, was in using entity-specific assumptions in arriving at a current value or using market participation assumptions. Japan has experimented with market prices and other forms of inflation-adjusted values for securities, but Kubota concludes by cautiously welcoming fair value and the heterogeneity it allows, over the existing current price mechanism based on exit values.

The problem is not the departure from the essence of accounting, or that the decision-usefulness conceptual frameworks are inherently iniquitous. What this book demonstrates (but does not explicitly acknowledge) is that the IASB's proposed methods for fair value accounting are just another episode in trying to find ways of communicating the reality of business transactions, or at least a means of providing workable representations for difficult-to-capture assets and liabilities. The argument about whether full fair value bases or historical cost bases are more desirable is not discussed in full, either. It is accepted that a mixed methods basis of accounting has become taken for granted, and that this is seen as largely deplorable. In Georgiou and Jack (2011), it is argued that the IASB movement toward fair value accounting has legitimized a mixed methods basis for financial reporting. They point out that "historic cost has only ever enjoyed an episodic legitimacy as the primary basis of accounting and that although the principles of FVA [fair value accounting] have been embedded and legitimized to an extent, the main effect has been a pragmatic acceptance that mixed measurements in financial reporting are inevitable" (Georgiou and Jack 2011, 321).

A lesson can be learned from the American pragmaticist philosophers, as Macintosh (2002) and Rutherford (2013) have pointed out and which I touched on above. For Dewey, Peirce, and James (the classical pragmatists) and the neo-pragmaticists, the question of truth is irrelevant: knowledge is fallabilist and probabilistic. Macintosh (2002) uses this to argue for a heteroglossic approach to financial reporting: more than one statement, and each statement produced using different bases of accounting or accounting policy. For Rutherford (2013), pragmaticism provides a strong epistemological underpinning to the classical program of normative research in financial accounting. The process of inquiry advocated by Dewey is one of conceptual thought, experiment, and reformulation where "the concepts, categories, distinctions and relations embodied in accounting practice are to be evaluated in terms of the functional fitness of observation and ideas in resolving the stress in financial reporting's problem situation" (Rutherford 2013, 213).

From my reading, this is the interesting story that *Fair Value Accounting in Historical Perspective* gives to us in setting out the problems that accountants have faced over time in trying to account for assets and liabilities, rather than the rationale against decision-usefulness and mixed methods to which it lays claim. Scandals and dissonance between users and standard setters are in fact the creative ground in which satisfactory solutions emerge—but they will always be only temporary solutions as organizations and societies themselves will change and alter what needs to be represented by accounting information.

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LISA JACK
Professor of Accounting
University of Portsmouth

EDITORIAL POLICY AND STYLE INFORMATION

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Baiman, S., and M. Rajan. 2002a. The role of information and opportunism in the choice of buyer-supplier relationships. *Journal of Accounting Research* 40 (2): 247–278.

Baiman, S., and M. Rajan. 2002b. Incentive issues in inter-firm relationships. *Accounting, Organizations and Society* 27 (3): 213–238.

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Footnotes: Footnotes are not used for documentation. Textual footnotes should be used only for extensions and useful excursions of information that if included in the body of the text might disrupt its continuity. Footnotes should be inserted using the "footnote" or "endnote" feature of the word processing software which will automatically number the footnotes throughout the manuscript with superscript Arabic numerals.

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Authors should note the following guidelines for submitting manuscripts:

1. Manuscripts currently under consideration by another journal or publisher should not be submitted. The author must state upon submission that the work is not submitted or published elsewhere.
2. For manuscripts reporting on field surveys or experiments: If the additional documentation (e.g., questionnaire, case, interview schedule) is sent as a separate file, then all information that might identify the author(s) must be deleted from the instrument.
3. New and revised manuscripts must be submitted through the Manuscript Submission and Peer Review System, located at http://accr.allentrack.net. The site contains detailed instructions regarding the preparation of files for submission. Please submit separate files for (1) the manuscript's title page with identifying information (not forwarded to reviewers), (2) the manuscript with title page and all other identifying information removed, and (3) any necessary supplemental files, such as experimental instructions and/or response memoranda on invited revisions.
4. A nonrefundable submission fee is required unless explicitly waived by the editor for invited revisions of previous submissions. The submission fee in U.S. funds is $200.00 for members and $400.00 for nonmembers of the AAA payable by credit card (VISA, MasterCard, or American Express only). The payment form is available online at: https://aaahq.org/AAAforms/journals/

tarsubmit.cfm. If you are unable to pay by credit card or have any questions, please contact the AAA Member Services Team at (941) 921-7747 or info@aaahq.org.

5. Revisions must be submitted within 12 months from the decision letter inviting a revision.

COMMENTS

The Accounting Review welcomes submissions of comments on previous *TAR* articles. Comments on articles previously published in *The Accounting Review* will generally be reviewed by two reviewers, usually including an author of the original article (to assist the editor in evaluating whether the submitted comment represents the prior article accurately) and an additional reviewer who is independent of the original article. If a comment is accepted for publication, the original author will generally be invited to reply. All other editorial requirements, as enumerated above, apply to proposed comments.

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MARCH 2015 PLACEMENT ADS

The deadline for free position ads to be included in this section of *The Accounting Review* is two months prior to the desired publication in the January, March, May, July, September, or November issues. Position ads, which are free with the purchase of a job posting in the AAA Career Center, should provide all relevant information about the available positions and must include contact information or application instructions for interested candidates. For more information on how to purchase a job posting in the Career Center, or for up-to-date, detailed information about the placement listings in this issue, please go to the AAA website at http://aaahq.org and click on "Career Center," or call our office at 941-921-7747.

TEXAS A&M UNIVERSITY–TEXARKANA seeks an outstanding leader to serve as dean of the College of Business, an appointment that is to be assumed on July 1, 2015. The University, a member of the prestigious Texas A&M University system, is being assisted by Academic Search, Inc. A full search profile providing additional information about the University as well as the position and required and preferred qualifications can be found under Current Searches at: http://www.academic-search.com. Applications should consist of a substantive cover letter addressing the qualifications listed in the profile, a *curriculum vitae* or resume, and a list of at least five professional references with full contact information; references will not be contacted without explicit permission from the candidate. Applications, nominations, and expressions of interest can be submitted in confidence to: TexarkanaDeanofBusiness@academic-search.com. The position will remain open until filled, but only applications received by January 12, 2015 can be assured full consideration. Confidential discussions about this opportunity may be arranged by contacting Senior Consultant Tessa Martinez Pollack at: tmp@academic-search.com.

CALIFORNIA STATE UNIVERSITY, SACRAMENTO, College of Business Administration invites applications from individuals interested in joining our dynamic and growing faculty beginning Fall 2015. California State University, Sacramento is accredited by The Association to Advance Collegiate Schools of Business (AACSB International). The College of Business Administration has a tenure-track faculty positions opening in Accounting. For further details, please visit: http://www.csus.edu/hr/faculty/bus/index.htm/. California State University, Sacramento is an Affirmative Action/Equal Opportunity Employer and hires only those individuals who are lawfully authorized to accept employment in the United States. Jeanne Clery Act statistics available.

BROOKLYN COLLEGE–CUNY, School of Business invites nominations and applications for appointment to the newly created Marshall G. Kaplan Chair in Municipal Government—Accounting. The position has tenured status at the Associate Professor level, CUNY Job ID 11822. The Chair offers a reduced teaching load, discretionary research funds, and an annual stipend. The successful candidate will be expected to teach and develop curriculum in all aspects of municipal government (e.g., local government accounting and budgeting, tax codes and regulation, efficiency and performance management), leading to programmatic opportunities for students; and interact with the local business and government communities, with an outcome of raising the visibility of the Brooklyn College–CUNY School of Business in the area of municipal government. Applicants must possess a Ph.D. or equivalent degree (i.e., D.B.A.) in accounting. Review of applications will begin November 17, 2014 and will continue until the position is filled. For more information and to apply, please visit http://www.brooklyn.cuny.edu/facultyjobs and scroll down to 11822. AA/EOE.

NATIONAL UNIVERSITY invites faculty applications in all areas at all ranks. Preferred candidates should have a completed Ph.D. or D.B.A., multi-area subject matter expertise, professional certification, accounting practical experience, demonstrated success with online curriculum delivery or a commitment to online education. Successful candidates should have an established research record, or show potential of establishing a research record, and demonstrated success or commitment to delivering relevant education that engages the students in the learning process in a manner that will be impactful on both the student and the profession in both a face-to-face classroom, as well as an online environment. National University is the second largest private non-profit university in California. NU offers courses in a one-course-per-month format both onsite and online. Submit application materials to: slightner@nu.edu. Provide *cv*, contact information for three professional references, evidence of teaching effectiveness and research, and any work experience.

THE UNIVERSITY OF CINCINNATI invites applicants for a tenure-track Accounting Professor (open rank) position in the Lindner College of Business, beginning Fall 2015. Candidates must have a doctorate in accounting from an AACSB-accredited institution, with consistent research productivity, including recent quality, refereed publications. Recent graduates and those with doctorates by July 2015 must demonstrate the ability to perform quality research and must be considered a "Scholarly Academic" under AACSB guidelines. Excellent teaching credentials are highly valued and quality teaching is expected; teaching load is commensurate with research productivity. Significant public or private accounting experience is highly valued, but not required, as are professional accounting certifications. Candidates must possess both written and spoken English fluency. To apply for position (214UC8704), go to: https://www.jobsatuc.com/applicants/jsp/shared/frameset/Frameset.jsp?time=1415804556486 [all ranks apply under Associate title]. For questions, contact: Robert.Larson@uc.edu. UC is an Affirmative Action/Equal Opportunity Employer and is a smoke-free work environment.

SAN JOSE STATE UNIVERSITY, SAN JOSE, is seeking applicants (Job Opening ID: JOID Number 23094) at the Assistant/Associate Professor rank (tenure-track). Teaching assignments are at both the graduate and undergraduate levels in traditional and accelerated formats. The primary disciplinary needs are in financial accounting at undergraduate and graduate levels. Secondary needs include auditing and managerial accounting. Conduct research, develop grants, present professional papers, and publish in peer-reviewed journals. Qualifications: Possess a Ph.D. and be academically qualified under AACSB standards (if ABD, completion of the doctorate will be required for employment and will be a prerequisite for tenure and promotion). Applicants should have awareness of and sensitivity to the educational goals of a multicultural population—in age, cultural background, ethnicity, primary language, and academic preparation—as might have been gained in cross-cultural study, training, teaching, and other comparable experience. Candidate must address the needs of a student population of great diversity through course materials, teaching strategies, and advisement. Demonstrate a record of prior teaching excellence at the university level. Professional practice and experience in Accounting with CPA licensure preferred. Demonstrate an ability to work in a collegial atmosphere as part of a team. Possess excellent communication and interpersonal skills. Salary Range: Commensurate with qualifications and experience. Starting Date: August 18, 2015. Eligibility: Employment is contingent upon proof of eligibility to work in the United States. For consideration by the first screening date, send a letter of application, *curriculum vitae*, statement of teaching interests/philosophy and research plans, and at least three original letters of reference with contact information by January 15, 2015 to Dr. Elizabeth Grace at elizabeth.grace@sjsu.edu.

AUBURN UNIVERSITY, The Raymond J. Harbert College of Business, School of Accountancy is seeking to fill one full-time tenure-track Assistant Professor position, contingent upon final budget approval, beginning no later than Fall 2015. While all areas of teaching and research will be considered, the areas of immediate need are AIS and financial. Candidates must have strong research credentials/potential and will be expected to publish in top-tier scholarly journals. The faculty member is expected to contribute in instruction, research, and service/outreach/extension. Candidates with professional certification are desired. Successful applicants must possess a doctorate in accounting, a degree from an AACSB-accredited institution is desired, prior to the date of employment. The successful candidate must meet eligibility requirements to work in the U.S. at the time the appointment is scheduled to begin and continue working legally for the proposed term of employment. Excellent communication skills are required. Review of applications will begin July 15, 2014 and continue until a qualified candidate is recommended for appointment. To be considered, qualified candidates must submit a letter of application, *curriculum vita*/resume, teaching evaluations, a current working paper or dissertation proposal, a copy of all relevant transcripts, and contact information for three references. All application materials should be submitted to: http://aufacultypositions.peopleadmin.com/postings/486. Auburn University is an EEO/Vet/Disability employer.

UNIVERSITY OF HOUSTON–CLEAR LAKE (UHCL), School of Business is seeking two faculty members at the rank of Assistant Professor of Accounting, beginning August 2015. A doctorate in Accounting or an ABD nearing completion from an AACSB-accredited school is required. Preference will be given to candidates who are conducting discipline-based research and willing to teach auditing, advanced accounting, or tax. Applications are accepted online at: https://jobs.uhcl.edu. To apply, please complete the application and attach a letter of interest and a *vita*. Please send three recommendation letters and a transcript from your doctoral institution to: Michael Lacina, Accounting Search Committee, University of Houston–Clear Lake, Box 212, 2700 Bay Area Boulevard, Houston, TX 77058-1098. Review of applicants begins December 8, 2014. The University reserves the right to extend the search or not to fill one or more positions. The candidate must be eligible to work in the United States. UHCL is an AA/EO employer.

EASTERN CONNECTICUT STATE UNIVERSITY has an opening for a tenure-track positon of Assistant/Associate Professor in Accounting for Fall 2015. Ph.D. or a combination of Master's degree with Accounting certification required. For details, please go to: http://www1.easternct.edu/humanresources/jobs/faculty-positions/ or contact Dr. Richard Silkoff at: silkoffr@easternct.edu.

TENNESSEE STATE UNIVERSITY invites applicants for Assistant/Associate/Full Professor of Accounting. Applicant will teach undergraduate and/or graduate courses on a nine-month contract, will perform research leading to publication in refereed journals, will participate in service activities supporting the mission of the Department, College, and University. Normal teaching load for Associate/Full Professor who meets college research and publication guidelines is nine semester hours. We seek a candidate with a commitment in working effectively with students, faculty, and staff from diverse backgrounds. Minimum Qualifications/Experience will include a Doctorate in Accounting or equivalent is required (ABD would be considered at the assistant level). Certification (CPA, CMA, CIA, etc.) with current public or industry accounting experience preferred. Applicants for Associate/Full must have evidence of excellent teaching abilities as well as a demonstrated record of research and refereed publication required. Apply at: https://jobs.tnstate.edu or contact Eva Jermakowicz at ejermakowicz@tnstate.edu.

UNIVERSITY OF CINCINNATI, Lindner College of Business invites applicants for an Educator track Accounting Professor (open rank) position beginning Fall 2015. Primary responsibilities involve teaching and related service activities. High-quality teaching is expected; teaching load and rank commensurate with teaching credentials, prior professional accounting experience, and prior research productivity. Sustained academic and professional engagement is required, and publishing in refereed journals is desirable. Required qualifications include professional accounting work experience and a professional accounting certification. Minimum educational qualifications include a master's degree in an accounting related area from a U.S. AACSB-accredited institution or a relevant graduate degree with significant experience in teaching and/or performing U.S. accounting. Preferred qualifications include a doctorate in accounting from an AACSB-accredited institution and experience teaching accounting courses at U.S. AACSB-accredited institutions with evidence of effective classroom outcomes. Candidates must possess both written and spoken English fluency, and provide evidence of such throughout the interview process. To apply for position (214UC0053) go to: https://www.jobsatuc.com/applicants/jsp/shared/frameset/Frameset.jsp?time=1416854527660 [all ranks apply under Assistant title]. For questions, contact Robert.Larson@uc.edu. The University of Cincinnati is an Affirmative Action/Equal Opportunity Employer/ M/F/Vet/Disabled. UC is a smoke-free work environment.

WESLEYAN COLLEGE, Department of Business and Economics invites applications for a tenure-track position at the Assistant Professor level beginning August 2015 (http://www.wesleyancollege.edu). A Ph.D. in accounting or its imminent completion preferred. We seek candidates who demonstrate a commitment to excellent teaching in a liberal arts setting. Our normal teaching load is seven courses per academic year, possibly to include one or two courses in our Executive M.B.A. program. The successful candidates will be expected to teach effectively, pursue research interests, advise students, and serve on college committees. We particularly welcome those with fields in taxation and financial accounting. Those interested in participating in our dual-degree program with Guangzhou University also encouraged. Minimum qualifications are a master's degree in accounting with at least 18 academic hours in accounting at the graduate level, and an active CPA license. We will begin interviews as soon as possible. Send letters of interest, *vitae*, transcripts and three current letters of recommendation to: Dr. Kel-Ann Eyler, Accounting Program Director, Dept. of Business and Economics, Wesleyan College, 4760 Forsyth Road, Macon GA, 31210-4462. Inquiries may be made via email to: keyler@wesleyancollege.edu. EOE

SOGANG UNIVERSITY, Sogang Business School invites applications for a tenure-track at all ranks (starting in Fall 2015) in the area of Accounting. Sogang University is located in Seoul, Korea, and Sogang Business School is accredited by the Association to Advance Collegiate School of Business (AACSB International). Candidates at advanced ranks must possess a doctoral degree, and candidates for Assistant Professor positions must possess or be near completion of a doctoral degree. Demonstrated research accomplishment, teaching and business experience, and professional certification are desirable. If interested, contact Professor Minsup Song, recruiting committee chair for more detailed information: Phone (+82) 10-3580-8172; Email: msong@sogang.ac.kr.

WESTERN NEW ENGLAND UNIVERSITY invites applications for a tenure-track Assistant/Associate Professor accounting position to begin August 2015. Candidates are expected to hold a doctorate in accounting or anticipate having completed all degree requirements by the starting date. The successful candidate will demonstrate a strong commitment to teaching in the undergraduate and graduate programs and to publishing scholarly research. All teaching areas will be considered. The position will also have the title of KPMG Community Relations Professor, which involves engagement in community outreach activities. Western New England University is a private, independent, coeducational institution founded in 1919 and located on an attractive 215-acre suburban campus in Springfield, Massachusetts within easy access of Boston and New York City. More information about the University is available at: http://www.wne.edu. Send *vita*, teaching evaluations, research manuscript, and reference information to: Dr. Julie Siciliano, Dean, College of Business, Western New England University, 1215 Wilbraham Road, Springfield, MA 01119. Electronic submissions are encouraged and should be sent to: deansofficeacctgen@wne.edu. Western New England University is an Equal Opportunity Employer.

TRUMAN STATE UNIVERSITY, School of Business invites applications for a full-time tenure-track position in Accounting to start August 2015. Rank depends on qualifications. For information on this or other positions visit: http://vpaa.truman.edu/positions. Truman State University, 100 E. Normal, Kirksville, MO 63501. Truman State University is an Affirmative Action/Equal Opportunity Employer committed to cultural diversity and compliance with the Americans with Disabilities Act.

ELON UNIVERSITY, Martha and Spencer Love School of Business, Department of Accounting invites applications for a tenure-track position in accounting at the Assistant, Associate, or Full Professor level. The specialization of immediate need is in intermediate and advanced financial accounting. This position will start in Fall 2015 (preferred). Elon faculty members are expected to make strong contributions to instruction and mentoring, research, and service. Teaching will include courses in Elon's General Studies Program. Final candidates will be expected to demonstrate effective undergraduate teaching. Applicants above the Assistant Professor level should have an established record of research publications and an ongoing research agenda. An interest in teaching in the graduate programs is desirable. Only candidates with an earned doctorate in accounting from an AACSB-accredited university will be considered. Professional accounting certification is a plus. Review of candidates will begin immediately and continue until the position is filled. To apply, send a letter of interest, *curriculum vitae*, statement of teaching philosophy, statement on research interests, information on teaching evaluations, and a list of three references to: accsearch@elon.edu (preferred), or to: Accounting Faculty Search Committee, Love School of Business, Elon University, 2075 Campus Box, Elon, NC 27244. Elon University is an Equal Opportunity Employer committed to a diverse faculty, staff, and student body and welcomes all applicants.

THE GEORGE WASHINGTON UNIVERSITY, School of Business, Department of Accountancy invites applications for the Benjamin Franklin Chair of Accountancy, to begin as early as Fall 2015. Applicants must have a Ph.D. in Accounting and should be a nationally recognized scholar with a very strong and consistent record of publications in top-tier accounting journals. Candidates must also exhibit excellence in teaching, including an interest in working with Ph.D. students. To be considered, please submit the following items online: cover letter, *curriculum vitae*, and a recent working paper. Please submit to: http://www.gwu.jobs/postings/25368. Review of applications will begin on January 19, 2015 and will continue until the position is filled. Only complete applications will be considered. Employment offers are contingent on the satisfactory outcome of a standard background screening. The university is an Equal Employment Opportunity/Affirmative Action Employer that does not unlawfully discriminate in any of its programs or activities on the basis of race, color, religion, sex, national origin, age, disability, veteran status, sexual orientation, gender identity or expression, or on any other basis prohibited by applicable law.

UNIVERSITY OF LOUISVILLE, College of Business, School of Accountancy invites applications for a full-time, tenure-track position at the level of Assistant Professor of Accountancy to begin August 2015. The successful candidate will have a Ph.D. (Accountancy) completed by August 1, 2015. Candidates must exhibit the potential to publish in top-quality accountancy journals, and are expected to demonstrate excellence in teaching and have effective interpersonal skills. While financial accounting is preferred, all areas of research and teaching will be considered. Salary and teaching loads are highly competitive. Louisville, Kentucky offers many of the attractions of a large city and has a thriving business community. Candidates must apply online at: http://www.louisville.edu/jobs, and upload a *curriculum vita*. The Job Opening ID number is 31282. The Search Committee will accept applications until a new Accountancy faculty member is selected. Interested individuals must submit separately their letter of interest, names of three references, student evaluations, statement of teaching philosophy and examples of scholarly work by December 31, 2014 to assure optimal consideration. Send information to: Dr. Ying (Julie) Huang, University of Louisville, School of Accountancy, 110 West Brandeis Avenue, Louisville, KY 40292 or electronically to: y.huang@louisville.edu. Additional information about the School of Accountancy and the College of Business can be found at: http://www.business.louisville.edu. Please see the "Job Description." The University of Louisville is an Equal Opportunity/Affirmative Action Employer.

EASTERN MICHIGAN UNIVERSITY seeks applicants for tenure-track Assistant Professor position beginning Fall 2015. Primary interest in managerial or financial accounting with secondary interest in other areas desired. The selected candidate must have a commitment to excellent teaching across multiple areas, research that leads to peer-reviewed publications and university service. Individuals must have at minimum an ABD with a major in accounting from an AACSB-accredited university or its equivalent, leading to a Ph.D. Ph.D. in Accounting from an AACSB-accredited program/school, CMA/CPA or other professional certifications desired. EMU is a public, comprehensive university of 24,000 students in Southeastern Michigan and the College of Business is AACSB-accredited. Contact Howard Bunsis at hbunsis@emich.edu.

TEXAS A&M UNIVERSITY–CORPUS CHRISTI, College of Business, Department of Accounting, Finance and Business Law invites applications for a tenure-track position at the Assistant/Associate Professor level beginning as early as Fall 2015, subject to final budgetary approval. The position is open to all areas in accounting. Applicants must have a doctoral degree in accounting from an AACSB-accredited institution and should have a strong commitment to scholarly research and teaching excellence. Certification and professional experience in accounting are preferred. Salary is competitive. Texas A&M University–Corpus Christi is a learning-centered institution. TAMUCC (The Island University) is located on its own island surrounded by the water of Corpus Christi and Oso bays. The Island University's federal designation as a Hispanic Serving Institution (HSI) provides a foundation for closing educational gaps, while its strategic location on the Gulf of Mexico and on the cultural border with Latin America provides a basis for gaining national and international prominence. The College of Business undergraduate and graduate business and accounting programs are accredited by AACSB International. The College is housed in the Michael and Karen O'Connor building, a state-of-the-art new facility. Applications must include (1) letter of application addressing qualifications listed above including a statement of teaching & research philosophy, (2) a *curriculum vita*, (3) research/publication record; (4) indicators of teaching activity/effectiveness or potential; and (5) a list of three references. Go to: http://www.cob.tamucc.edu/ for more information on the position, the College, and the University.

THE UNIVERSITY OF TEXAS AT RIO GRANDE VALLEY invites applications for Assistant, Associate, and Full Professors of Accounting. Positions are available at the Brownsville and/or Edinburg campuses. Faculty members are responsible for teaching undergraduate and graduate accounting courses; engaging in high-impact scholarly/research activities; and participating in the School, College, and University committees and activities. Teaching area is flexible. Minimum Requirements include a Ph.D. or D.B.A. in accounting from an AACSB International-accredited institution; must be qualified under AACSB guidelines, have a substantial record of scholarship or demonstrate a potential for scholarship, be an effective teacher, and have effective communication and organizational skills. For Assistant Professor Positions, candidates must have a Ph.D. or D.B.A. in Accounting from an AACSB-accredited university by August 2015. UTRGV is a consolidation of UT Brownsville and UT Pan American with a mission to become a tier-1 research institution. Application must include a *curriculum vitae*, list of three references, a letter of interest, and copies of all undergraduate and graduate transcripts. Complete all sections on the application. If you are applying for the first time, please complete all biographical information including address, email, and phone. You may update this at any time by selecting to edit your profile in the application. Security sensitive; conviction verification conducted on applicant selected. Please complete an application at: https://www.careers.utrgv.edu.

UNIVERSITY OF CENTRAL FLORIDA, Kenneth G. Dixon School of Accounting invites applications for the Kenneth G. Dixon Eminent Scholar Chair in Accounting. The appointment will begin August 2015. We are interested in an individual who can contribute to our existing research strengths and our Ph.D. program. Candidates should possess a Ph.D. specializing in accounting and have an internationally recognized record of research evidenced by publications in premier accounting journals. Candidates should also have demonstrated excellence in teaching and service. The primary research focus for our faculty and Ph.D. program is behavioral, public policy, ethics, and systems. The Dixon School of Accounting has separate AACSB-accreditation. In addition to the Ph.D. in Accounting degree, the department offers a Master's of Science in Accounting (M.S.A.), a Master's of Science in Tax (M.S.T.) degree, and undergraduate degrees in accounting. For more information on the College of Business Administration, see our website at: http://www.bus.ucf.edu/. Consideration of applicants will begin immediately, and applications will be accepted until the position is filled. The desired start date for the position is August 2015. Applicants should submit a *curriculum vita*, a cover letter stating interest and qualifications, and the names, addresses, and telephone numbers of at least three professional references to the UCF Human Resource Department at: http://www.jobswithucf.com/postings/40526. Please contact Dr. Vicky Arnold (Vicky.Arnold@ucf.edu), Search Committee Chair, if you have questions. UCF is an Equal Opportunity/ Affirmative Action Employer.

FLORIDA ATLANTIC UNIVERSITY, College of Business invites applications for two tenure-track positions in Accounting at the Assistant/Associate/Full Professor rank beginning August 2015. The FAU College of Business is accredited by the AACSB. For appointment at the rank of Assistant Professor, candidates must have earned a Ph.D., or be close to completing the degree, and exhibit strong potential for and commitment to excellence in research and teaching. Associate and Full Professor applicants should have a proven track record of exceptional scholarship and distinguished leadership. Preference will be given to applicants with interests in financial accounting, auditing, or taxation (although all areas of expertise will be considered). Salary and research support are competitive. A highly qualified experienced hire is highly desired. Application materials including a letter of interest, *curriculum vitae*, evidence of teaching competence, a sample of working papers, and three references should be submitted online at: https://www.jobs.fau.edu (Position Numbers 992960 and 990122). Questions about these positions may be addressed to the Accounting Recruiting Committee, Email: jcao@fau.edu. Review of applications will continue until the positions have been filled. A background check will be required for candidates selected for the positions. Florida Atlantic University is an Equal Opportunity/Equal Access Institution.

THE UNIVERSITY OF IOWA, Department of Accounting, Tippie College of Business invites applicants for a one-year visiting appointment beginning August 19, 2015. We will consider candidates at all ranks. Preference will be given to experienced candidates who have taught financial accounting at the undergraduate level. Required qualifications include a demonstrated interest in and capacity to do research publishable in top-tier accounting journals, a high level of teaching competence, and an earned doctorate. We offer a competitive compensation package. We will screen applicants at all ranks on an ongoing basis. To apply, please go to: http://www.jobs.uiowa.edu/jobSearch/faculty/ and reference Requisition # 65827. You will be asked to upload a resume, representative research output, evidence of teaching capabilities; and to provide the names for at least three letters of recommendation. All qualified applicants are encouraged to apply and will receive consideration for employment free from discrimination on the basis of race, creed, color, national origin, age, sex, pregnancy, sexual orientation, gender identity, genetic information, religion, associational preference, status as a qualified individual with a disability, or status as a protected veteran.

THE PENNSYLVANIA STATE UNIVERSITY HARRISBURG, School of Business Administration invites applications for a tenure-track Assistant Professor of Accounting in its AACSB-accredited program starting Fall 2015. Doctorate in Accounting or related discipline from AACSB-accredited school preferred; ABDs completing doctorate by August 2015 will be considered. Faculty members are expected to pursue active research, teach 3/2 load first two years, then 3/3 load. Ability to teach graduate/undergraduate courses in accounting information systems is preferred; a financial accounting specialty will also be considered. A secondary area of expertise is a plus and some online course delivery is likely. Strong commitment to effective teaching and publication is required. Professional certification (CPA, CMA) and prior teaching, research, professional experience preferred. Compensation is AACSB competitive. For information about safety at PSU, and to review the Annual Security Report that contains information about crime statistics and other safety and security matters, please go to: http://www.police.psu.edu/clery/, which will also provide you with detail on how to request a hard copy of the Annual Security Report. For additional details about PSU, please visit our website at: http://www.hbg.psu.edu. Review of applications will begin immediately and continue until the position is filled. Applicants should upload a cover letter, *curriculum vitae*, and contact information for four references to: http://www.psu.jobs, job number 51870. In addition to the uploaded materials, supplemental materials (reference letters, transcripts, syllabi, etc.) should be mailed to: ATTN: Assistant Professor of Accounting, Job # 51870, Penn State Harrisburg, School of Business Administration, E355 Olmsted, Middletown, PA 17057. For more information, please contact Dr. Tom Buttross at: teb11@psu.edu. Penn State is an Equal Opportunity/Affirmative Action Employer, and is committed to providing employment opportunities to minorities, women, veterans, disabled individuals, and other protected groups.

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